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LEADING AMERICAN STATISTICIANS IN THE NINETEENTH CENTURY

PAUL J. FITZPATRICK
Catholic University of America

INTRODUCTION

THIS article aims to present leading American statisticians in the nineteenth century and to indicate their contributions to the field of statistics. Other disciplines have already developed such a history, one example being that of economics [14], [18]. Yet so far, no history has ever been written about American statistical thought.

The winning of our independence in 1783 brought forth many problems, political, social and economic. To solve these problems we had at that time very little statistical data and records available. Hence it was a difficult situation to solve these problems objectively. It became necessary to take an inventory of our resources, both human and material. Our earlier federal censuses were chiefly concerned with only a few phases of our population. Thus a number of statistical almanacs or yearbooks gradually appeared such as: *A View of the United States of America* (Philadelphia, 1794) by Tench Coxe, Assistant Secretary of the United States Treasury; *Thoughts on the Increasing Wealth and National Economy of the United States of America* (Washington, 1801), and *Economica: A Statistical Manual for the United States of America* (Washington, 1806) both by Samuel Blodget, architect, economist and merchant; *A Statistical View of the Commerce of the United States of America* (Hartford, 1816), a second edition (New York, 1817), and a third edition (New Haven, 1835) by Timothy Pitkin, a prominent Federalist congressman from Connecticut, regarded as an authority on the political history of the country. Congress authorized the purchase of 250 copies; *The Resources of the United States of America* (New York, 1818) by Rev. Mr. John Bristed, editor, lawyer, and Episcopal clergyman; *Statistical Annals* (Philadelphia, 1818) by Adam Seybert, M.D., democratic congressman from Pennsylvania. Congress authorized the purchase of 500 copies; *Statistical, Political and Historical Account of the United States of North America* (3 volumes, Philadelphia, 1819) by David B. Warden, author and diplomat; *View of the United States: Historical, Geographical, and Statistical* (Philadelphia, 1828) by William Darby, a surveyor and one of America's leading geographers for a quarter of a century. *Tabular Statistical Views* (Washington, 1829 and 1833) by George Watterston and Nicholas Biddle Van Zandt, "published under the patronage of the Congress of the United States." Congress

authorized the purchase of about 800 copies of the 1829 edition and 750 copies of the 1833 edition; and *A Complete Descriptive and Statistical Gazetteer of the United States of America* (New York, 1843) by Daniel Haskel, formerly president of the University of Vermont and John C. Smith, a geographer.

The foregoing titles treated the national statistical picture. A few statistical works dealing with individual areas were published such as *A Statistical Account of the City of New Haven* (New Haven, 1811) by Rev. Timothy Dwight, President of Yale College, under the auspices of Connecticut Academy of Arts and Sciences; *A Geographical and Statistical View of Massachusetts proper* (Greenfield, 1813) by Rudolphus Dickinson of Massachusetts and later a congressman from Ohio; *A Statistical View of the District of Maine* (Boston, 1816) by Moses Greenleaf; *Statistics of South Carolina* (Charleston, 1826) by Robert Mills, engineer and architect, *A Statistical Sketch of the State of Michigan* (New York, 1838) which gives no author, and *Statistics of the State of Georgia* (Savannah, 1849) by George White.

LEMUEL SHATTUCK (1793–1859)

The first outstanding American statistician of the nineteenth century is Lemuel Shattuck, founder of the American Statistical Association, whom Walter Willcox has called "the most influential American statist." He was also a leading legislator and genealogist [55], [56].

Born at Ashby, Mass., the fifth among six children, he received a brief schooling at New Ipswich, N. H., about five miles away, where his parents had moved during his infancy, for a short period at Appleton Academy, as well as by diligent private study. Taught school at Troy and Albany, N. Y. and in Detroit, Michigan. About age 30, he returned to Concord, Mass. to operate a store, forming a partnership with his brother, Daniel [2, Vol. 5, 484]. Ten years later, about 1833, he moved to Cambridge and soon after, in 1834, to Boston where he became a bookseller and publisher. In 1839 at the age of 46, he retired from business to engage in public life [9, Vol. 17, 33–34]. Later in the same year, 1839, he played a very considerable part in the formation of the American Statistical Society (later changed to Association) in Boston, November 27, 1839, and he is generally regarded as its founder. He served the society as its Secretary, 1839–1844; Librarian, 1840–1843; and Counseler, 1844–1847 and 1854–1858 [13, Vol. 14, 16].

Shattuck became interested in statistics while preparing a book *A History of the Town of Concord* (Boston, 1835), one of the chapters of which deals with statistical history. He discovered that imperfect records were kept of births, marriages, and deaths by various localities, and he began to enlist the support of the state legislature as well as the Massachusetts Medical Society and the American Academy of Arts and Sciences for a more effective system of registration. This was finally achieved in 1842 by a law requiring more exact registration. Shattuck, moreover, as a member of the state legislature in 1849, was selected as chairman of a special committee which brought about a thorough revision of the state registration laws. Later, other states began to follow this plan of registration.

Even before this important event, Shattuck was demonstrating his bent for adequate statistical records, as Willcox points out:

> While on the Concord school committee, he had reorganized the public school system, prepared and printed school regulations, introduced school registers to be kept by

> teachers and filed as part of their report, secured the passage of an ordinance requiring an annual report in writing from the school committee to the town meeting, and had written the first such school report ever presented in Massachusetts. Other towns soon demanded the like and a few years later the legislature required them of school committees throughout the State [55].

Shattuck became a member of the Common Council of Boston during the years 1837–1841, and began to urge strongly the serious need of a census, particularly since the federal census of 1840 contained serious errors. He was appointed to direct this statistical undertaking, which was published as a volume of 274 pages in 1846 under the long title, *Report to the Committee of the City Council Appointed to Obtain the Census of Boston for the year 1845*. A singular contribution was the reporting for the first time in this country of "the name and description of every person enumerated . . . among other characteristics specifying the birthplace of each, and thus distinguishing the native from the foreign population." Another distinguishing feature of this report was the inclusion of an interpretative introduction, as has now become standard practice. Shattuck thus attracted national attention and was later called to Washington to assist in the preparation of the 1850 federal census. Holt reports:

> The views of the Superintendent of the Census, in which he attributes most of the ideas to Mr. Shattuck, who had taken a census of Boston in 1845, may be found in the Compendium of the Seventh Census, p. 13 [19].

Wright also indicates other ideas of Shattuck:

> Mr. Shattuck, of Boston, also recommended that a central board of three persons, as commissioners, should be organized at Washington, to be selected "not for their political opinions, but for their scientific attainments and knowledge of the matters they are to investigate," and to have the whole management of planning and carrying into execution all matters relating to the census. It was also a part of the plan that similar commissioners of three competent persons should be appointed by the central board in each state, with the consent of the Governor thereof, and that . . . by "This machinery a more perfect collection of facts could be obtained than in any other way" [62].

Shattuck drafted five of the six schedules for the 1850 federal census and also wrote the instructions for the enumerators. Moreover, "the feature of recording the name and description of every person enumerated, was proposed by this gentleman, and was first adopted in the Boston census of 1845" [5].

In 1849, Shattuck was chairman of a commission which made a sanitary survey of Massachusetts. This important statistical undertaking resulted in the *Report of a General Plan for the Promotion of Public and Personal Health* of 544 pages published in 1850. This report, containing fifty important recommendations, is highly regarded as "a milestone in the improvement of American public health" [55], [56] and resulted in the formation of the Massachusetts State Board of Health about 1869—to be followed later by other states. Shattuck was particularly interested in public health because his father and mother and two sisters had died of consumption. Moreover, he was impressed by Edwin Chadwick's article "Life Assurance—Diminution of Sickness and Mortality" in an 1828 issue of the *Westminster Review*.

In all his writings, Shattuck showed considerable familiarity with the best statistical practices in Europe, where standards were rapidly improving and were ahead of this country. He was in correspondence with various leading European scholars, particularly Adolphe Jacques Quetelet, the famous Belgian statistician, Sir Rawson William Rawson, Honorary Secretary and later President of the London Statistical Society, and Thomas H. Lister, the first Registrar General of England. This is manifested, as one example, in the 1842 revision of the Massachusetts registration system which followed closely the English practice. Thus, America is greatly indebted to Shattuck for his many energetic and scientific efforts to place population statistics on a high standard. Shattuck was also the author of two other works: *The Vital Statistics of Boston*, embracing thirty-five pages, published in 1841, and *Blank Book Forms for Family Registers* consisting of fifty-six pages, published in 1856.

EDWARD JARVIS (1803–1884)

Our second leading American statistician is Edward Jarvis, M.D., the fifth of seven children, who was born in Concord, Mass., and received his early education in the schools of Concord and in Westford academy [2, Vol. 3, 406]. Graduating from Harvard College in 1826, he attended the University of Vermont as a medical student and completed his studies at Massachusetts Medical College (now Harvard Medical School), receiving the M.D. degree from the latter institution in 1830 [9, Vol. 9, 621–622]. Jarvis practiced medicine first in Northfield, Mass., and then in 1837 in Louisville, Ky., remaining there until 1843, when he returned to Dorchester, Mass., where he spent the remainder of his life specializing in mental diseases, ably assisted by his devoted wife [13, Vol. 8, 373–374], [38, Vol. 2, 472–473], [39], [35, Vol. 12, 116], [57].

Being deeply interested in the treatment of insane patients, he studied vital statistics, particularly the federal census of 1840, wherein he found many serious errors in the statistics of insanity among the northern Negroes. Indeed, many northern towns were reported to have more Negro insane than total Negro population. Thus, he published a pamphlet entitled *Insanity Among the Coloured Population of the Free State* (Philadelphia, 1844), which originally appeared in the January, 1844 issue of *American Journal of the Medical Sciences* and revealed the seriousness of these errors. He strongly influenced the American Statistical Association, of which he was a member and later its third president for many years (1852–1882), to appoint a committee, which consisted of William Brigham, J. Wingate Thornton, and himself, to present a memorial to Congress in 1843 relating to these many census errors [19], [62, 37–38], [21].

Congress, however, because of the slavery question, took no action. Because of his knowledge of vital statistics, he was consulted in 1849 by the chief of the 1850 federal census (known then as the superintending clerk), Joseph C. G. Kennedy, who served as secretary of the Census board, which, under an act of Congress of March 3, 1849, consisted of the Secretary of State (who had previously been entrusted alone with the census), the Attorney General, and the Postmaster-General.

As Wright points out:

> This board, . . . in the course of its work, called into consultation many eminent statisticians, including, among others, Mr. Shattuck, Mr. Capen, Dr. Chickering, and Dr. Jarvis, and its plan was afterwards submitted to the Senate committee, by whom its principal features were adopted [62].

Jarvis was appointed by the Secretary of the Interior to prepare and edit the vital statistics of the 1860 federal Census, published in volume IV, thus placing vital statistics on a more comprehensive and scientific basis. Moreover, he was consulted later by General James A. Garfield, the able Chairman of the House of Representatives special Committee on the Census, and many of his recommendations were incorporated in the Committee's report. He also rendered valuable census assistance to General Francis A. Walker, Superintendent of the 1870 federal census.

As a member of the commission exploring and studying the problem of handling the insane and idiots, he prepared a six-hundred page report in 1855 which attracted wide attention [22], [9, Vol. 9, 622]. A publication as recent as 1953, *Psychosis and Civilization*, by Goldhamer and Marshall, praises highly this report of Jarvis [17].

He carried on an extensive correspondence, which he estimated to amount to one hundred thousand pages. Moreover, Koren reveals:

> It may truly be said that no human interest was foreign to Dr. Jarvis. In his speeches and writings on statistical subjects he traversed the widest fields and illuminated many an unexplored corner. . . . For thirty-one years he carried the heaviest load for the [American Statistical] Association. It is on record that during this time he prepared more than thirty-five papers and addresses for the meetings.

Upon the retirement of Jarvis as president, the American Statistical Association adopted the following resolution:

> Earnest expression of the manifold service he rendered the Association by the able manner in which he has performed the duties of his office, by the ability with which he has represented the society at home and abroad, by the many valuable papers he has prepared for us and read at our meetings, and by the assiduous and unwearied zeal with which he had labored at all times and in all ways for the advancement of the interests of the Association and of statistical knowledge [23].

He represented The American Statistical Association at the International Statistical Congress held in 1860 in London. When the International Statistical Congress was held in 1872 at St. Petersburg, Jarvis was again delegated to represent the Association but was unable to go. During his lifetime he collected a wide variety of works relating to statistics of which he gave more than 600 volumes to the American Statistical Association, which later on designated this collection as "The Jarvis Statistical Library." It was transferred in 1898 to the Boston Public Library. He was, furthermore, a member of many learned societies.

It can therefore be observed that Jarvis played a very prominent part during his lifetime in the improvement of the federal censuses relating to vital statistics, being assisted by his former townsman, Lemuel Shattuck. Finally, statisticians in general, and the American Statistical Association in particular, are greatly indebted to Jarvis for his untiring efforts and indefatigable zeal in keeping the American Statistical Association alive during the troubled years of this nation. He enjoys the unique distinction of serving as president of the Association (1852–1882) longer than any other person.

JAMES D. B. DEBOW (1820–1867)

James Dunwoody Brownson DeBow, editor and statistician, the first of five children, was born in Charleston, South Carolina, where he obtained his early education in the schools of that city [9, Vol. 5, 180–182]. His father, once a prosperous merchant, suffered heavy financial losses, and when DeBow was six years of age, his father died leaving the family in poverty [13, Vol. 5, 30–31]. At ten years of age DeBow went to work in a wholesale grocery house where he remained for seven years. When eighteen, he spent about a year at the Cokesbury Institute preparing for college [25]. He then entered the College of Charleston in 1839, graduating in 1843 as valedictorian of his class [11], [44], [53]. He studied law and was admitted to the bar within a year [40, 233].

DeBow became associated in 1844 with the *Southern Quarterly Review*, a very important periodical in its day. He wrote a number of important articles, and soon became its editor. He attracted both national and international attention by his article on "The Northern Pacific: California, Oregon and the Oregon Question," in the July 1845 issue, which was discussed widely in the United States and Great Britain and was even the subject of a debate in the French Chamber of Deputies [16]. In November 1845 he was a delegate to the Memphis Convention, presided over by the well-known statesman, John C. Calhoun, which discussed internal improvements of the South, and DeBow was appointed one of the Convention's secretaries. These discussions influenced him to move to New Orleans and to found a new magazine which appeared in January 1846 with the title *The Commercial Review of the South and West*, more popularly to be known as *DeBow's Review* [24, 401–402]. He expressed the intention "to collect, combine, and digest in permanent form, for reference, [the] important statistics" [6, Jan. 1846, 4] relating to the South and West. This *Review* later had the largest circulation of any periodical in the South and became very influential. At times, moreover, it had different titles [29].

This struggling review was kept alive in the early years only through staunch financial support of Maunsel White, a poor Irish immigrant who had become a rich New Orleans sugar planter and merchant. *DeBow's Review* consisted of forty-three full volumes from January 1846 to June 1880 [33], although it was handicapped by limited finances and by the Civil War, so that some numbers were not published. James Ford Rhodes, in his monumental *History of the United States from the Compromise of 1850*, and Professor U. B. Phillips, in his writings on the South, both found it valuable source material. Moreover, fifty-five southern senators and representatives in Congress endorsed it for the "ability and accuracy of its exposition of the working of the system of polity of the Southern States" [36], [41]. General Robert E. Lee also praised it [6, March 1866, 331]. This *Review* in its earlier years was devoted mainly to statistical and mercantile subjects, but later on became predominantly literary. The *Review* contained many general-purpose statistical tables collected from many sources. DeBow wrote some articles dealing with statistics. Several articles entitled "Statistical Bureaus of the States" reported the statistical activities in the several states. Another article, "The Science of Statistics," in the March 1847 issue, was a brief sketch of the origin of statistics. In the March 1848 issue he had an article entitled "Universities in America" in which he discussed and urged the teaching of commerce and statistics. This advocacy of

commercial subjects, as a basis of instruction, was the forerunner of our Collegiate Schools of Business, the first of which was the Wharton School of Finance and Economy, established in 1881 at the University of Pennsylvania.

Maunsel White collected funds "to secure an endowment for a *Chair of Commerce, Public Economy and Statistics*" at the University of Louisiana, now Tulane University. The minutes on page 18 of the Board of Administrators of the University of Louisiana record a motion unanimously adopted on February 13, 1848, nominating James D. DeBow to be "a Professor of Commerce and Statistics." The minutes of April 25, 1848 (page 27) record "Mr. DeBow's letter applying for a Library for his Professorship," and on May 24, 1848, the minutes show $500 appropriated for the purchase of books for the university, part of which was "for works adapted to the professorship of Commerce and Statistics." DeBow was to teach "Statistics of Population and Wealth in their application to Commerce, Agriculture, and Manufactures," but he failed to attract students [15], [16], [3].

In 1848 DeBow was appointed head of the newly-created Bureau of Statistics of the State of Louisiana where he attracted national attention by his careful and painstaking work. The bureau was in existence only three years because of the failure of the legislature to continue appropriations. Part of his activities as head of the Bureau of Statistics may be observed in a thirty-six page pamphlet entitled *Introductory to the First Report of the Bureau of Statistics of the State of Louisiana* which was reprinted in *DeBow's Review*. In the May 1850 issue of the *Review* he states on page 422:

> A very small edition of this report only having been published, and that with many typographical errors, the author trusts a publication in the Review will not be without interest to its readers.

Subsequent reports appeared in at least nine different issues of his *Review* during the years 1850, 1851, 1852, and 1853. Although the appropriations from the state legislature were small, he endeavored to obtain comprehensive statistical data of the State of Louisiana under thirteen major categories. The act of March 15, 1848 required:

> Information relative to the population, agricultural and other products, resources and commerce of the state, the mechanic arts, public education, public health and manufactures, and such other information as may be deemed important.

While thus engaged in this statistical undertaking, he conducted correspondence in 1849 with the federal census officials who were preparing for the 1850 census. They were very favorably impressed with his ideas and work in Louisiana and with his discriminating advice relative to the methods of taking a federal census.

The 1850 federal census, the Seventh, was planned at first by a Census Board through an act of March 3, 1849, and later in the next session of Congress by a special committee whose secretary, Joseph C. G. Kennedy, was placed in charge of census planning. Among eminent statisticians invited to give advice, DeBow made some constructive suggestions. When Kennedy, a Pennsylvania Whig, was not continued in office because of a change in administration, Franklin Pierce, the incoming Democratic president, appointed DeBow to the position,

and he reorganized the census work and carried it on until its completion in 1855. He not only accomplished the excellent editing of the census report of 1850 but also the supervision of a very informative compendium of that census entitled *Statistical View of the United States*, of which the United States Senate authorized 150,000 copies to be printed [35, Vol. 8, 161–162]. At the same time he continued to publish his *Review*.

The 1850 census is generally regarded far superior to the six previous censuses, which were of little value. As Wright observes:

> The methods in vogue at the first six censuses, therefore, were somewhat crude and essentially primitive, and the results presented covered but comparatively few details [62].

The procedures employed in the 1850 census were also followed in the 1860 and the 1870 censuses. DeBow's *Statistical View of the United States* is an excellent condensation of the census materials appearing in the quarto volumes of about 1200 pages. He makes therein a number of excellent recommendations. He strongly urges the use of trained enumerators in place of inexperienced workers who have been generally appointed by political influence (p. 17), and warmly recommends the establishment of a permanent office of the census at the seat of the government instead of the usual policy of setting up an organization every ten years before the taking of the census and then dismantling it (p. 18). He argued that we can not expect to continue a permanent staff of skilled clerks and supervisors, as is being done in many European countries, under temporary arrangements established anew every ten years. It was not until 1902, however, that a permanent Bureau of the Census was finally established in the United States.

DeBow also utilized the services of specialists in the writing of census reports; among them were John J. Abert, a topographical engineer, Alexander D. Bache, attached to the Coast Survey, Lorin Blodget and Charles C. Jewell, of the Smithsonian Institution, and others.

DeBow's fame as a statistician rests upon three chief activities [10, vol. 2]: first, head of the Bureau of Statistics of the State of Louisiana in 1848–1851; second, superintendent of the 1850 federal census; and third, owner and publisher of *The Commercial Review of the South and West* (1846–1867). Skipper, who has made a careful study of DeBow as a statistician, points out:

> In the absence of professional training, DeBow was forced to rely as a statistician upon an abundance of common sense, a healthy suspicion, a sound judgment, broad knowledge, and amazing industry. . . . Discriminating contemporaries placed unmistakable stamps of approval upon the statistician. Judged by the standards of his rivals the modern critic must give him a high rating [45].

He also points out that:

> the collection and classification of facts were major objectives of the editor. To others were left the task of examining his group of facts, of discovering mutual relationships, and of drawing therefrom whatever significance might be warranted [45].

Yet, as chief of the Louisiana Bureau of Statistics, Skipper again indicates that DeBow:

sought to gather not merely information but rather concrete bases for making analyses of relationships and of causes and effects. In the past he had been occupied chiefly with the collection of information, now he would analyze, draw conclusions, perhaps of interest, from dry facts [45].

His contributions to the Seventh Census (1850) are well described by Skipper in his other article [46].

There can be no question that DeBow was one of the most important statisticians of this century. General Francis A. Walker, superintendent of both the 1870 and 1880 federal censuses, considered him "one of the most meritorious of the earlier generations of American statisticians" [49]. Finally, Walker was influential in getting Congress to pass in two acts, March 3, 1879, and April 20, 1880, certain recommendations of DeBow contained in his *Compendium*.

FRANCIS AMASA WALKER (1840–1897)

Francis Amasa Walker, administrator, college professor, economist, journalist, military leader, and statistician, the youngest of three children of Professor Amasa Walker, a leading American economist, was born in Boston [47]. He received his early education in public and private schools in North Brookfield, Mass., and then attended Leicester Academy [12], [9, Vol. 19, 342–344]. Graduating from Amherst College as a Phi Beta Kappa in 1860 with A.B. and two prizes (having lost a year there because he was forced to stay home with a temporary weakness of the eyes), he spent about a year studying law in the office of Devens and Hoar. He entered military service in the winter of 1861–1862 as a sergeant-major and served with much distinction until illness forced his retirement in the spring of 1865 with the brevet rank of brigadier-general [13, Vol. 15, 323–325]. He taught Greek and Latin and mathematics at Williston Seminary for three years and then joined the editorial staff of the *Springfield* (Mass.) *Republican* serving until January 1869; at the age of 29, he was appointed chief, Bureau of Statistics, and Deputy Special Commissioner of Revenue through the good influence of David A. Wells, Special Commissioner of Revenue, who later became a leading economist [24, Vol. 7, 471–472].

In reorganizing the Bureau of Statistics on a scientific basis, he attracted attention as a statistician and was made superintendent of the 1870 federal census [26]. He resigned from the Bureau of Statistics because of the failure of Congress to continue appropriations and was appointed Commissioner of Indian Affairs in November 1871 to solve difficulties arising from scandals. At the same time, he continued to serve as superintendent of the Census without remuneration. He held these positions until December 1872, when he resigned to teach economics and statistics at Yale College. While at Yale, he lectured part-time at Johns Hopkins University in 1877 and 1878, being the first teacher of economics at the latter institution, which had opened its doors in 1876 [15], [61].

His census work of 1870, especially that dealing with the Negro population, has been criticized, but Walker had to work with untrained enumerators appointed with political influence and with an obsolete census machinery. Everyone conceded his ability as a statistician. He did not hesitate to admit some of the errors of his census work.

Walker served with distinction as Chief of the Bureau of Awards at the Philadelphia Centennial Exposition of 1876. He was appointed superintendent of the 1880 federal census, this time under a new law permitting him to appoint

census enumerators—an excellent innovation, this census resulting in the publication of twenty-two quarto volumes, a real mine of statistical information. It is generally conceded to be the best census up to that time. As a result, Europe came to regard Walker "as a statistician of the highest order" [7]. Moreover, Dorfman mentioned that Walker "soon was known as the first statistician of the land" [10, Vol. 3].

While teaching at Yale he published a number of works in economics, including *The Wages Question* (1876), *Money* (1878), and *Money in its Relations to Trade and Industry* (1879). In November 1881, he resigned from the census to accept the presidency of Massachusetts Institute of Technology, which he held to the time of his death. Even while president, he spent some time in Washington assisting in the completion of the census. This enormous statistical undertaking was more than nerve-wracking; Charles W. Seaton, Walker's successor, "was literally killed by the work, and three successive chief clerks of that census died in office" [51]. During this period he published *Political Economy* (1883), *Land and its Rent* (1883), *Political Economy: Briefer Course* (1884), *Money, Trade and Industry* (1889) and *International Bimetallism* (1896). These books, together with his articles, established him as one of the foremost American economists in the nineteenth century [38, Vol. 3, 649–650].

In the field of statistics, he made some important contributions, as is indicated by the census volumes. One government publication under his editorship, entitled *The Statistical Atlas of the United States* (1874), embracing the results of the 1870 census, contained excellent contributions from many scholars, both in the government and in the academic world, and attracted the attention and admiration of statisticians, both national and international. This pioneering Atlas, making skillful use of colors and graphic presentations, took advantage of recent developments in color printing and photography, and was the forerunner of wider employment of charts in the future. An eminent critic in the New York *World* declared it was "the handsomest and most useful work of its kind ever issued from a government office" [34].

Michel Chevalier, a well-known French geographer, said:

> By the arrangement you have adopted the results of the 9th Census are made clear to the eye even of the ignorant. The old way of presenting statistics only by figures is very inferior [34].

Spencer Baird, Secretary of the Smithsonian Institution, declared:

> You need not be told that it is the best thing of the kind ever attempted for a graphic representation of a vast multitude of important facts [34].

Because of the Atlas, the International Geographical Congress of Paris in 1875 awarded him a medal of the First Class.

Walker's influence as president of the American Statistical Association, succeeding Jarvis, from 1883 until his death in January 1897, was far-reaching, and he cooperated in many ways with the International Statistical Institute, of which he was one of the vice presidents.

By his addresses and writings he strongly stressed the importance of training statisticians. In one article he states:

> Unfortunately, while this rapid development of historical and economic work has been going on, a branch of study which has the highest virtue at once to train the hand of the historical or economic scholar and to furnish him with professional tools of the first importance has been almost wholly neglected. I refer to statistics, whose very methods are hardly known to the great majority of our economists and historians; and which is still to have its first chair founded in an American college. . . . It cannot be long, however, before the growing interest in economics and history will compel the recognition of statistics as a distinct and an important part of the curriculum of every progressive institution. The main difficulty will be to find the men who have had the training at once severe and liberal, which will qualify them to inspire and direct these studies [50].

In his last address before the annual meeting of the American Statistical Association, five days before his sudden death, he made some compelling remarks.

> The United States have spent millions and tens of millions upon the collection, compilation, and publication of statistics and yet they have never spent, perhaps $10,000 —certainly the government has never spent anything—in training and preparing the men who should conduct the statistical service of the country. . . . I do not believe that it is at all an exaggeration of the fact to say that if one per cent of what the United States Government has spent upon statistics had been devoted to the training and preparation of men to conduct our statistical service, it would haves saved at least a third of the cost of all the statistics collected in the past, and would have enhanced and improved the quality of the results almost indefinitely. . . .
>
> Our statistical work, national, state, and municipal, has fallen into the hands, almost without exception, so far as my limited knowledge extends, of men of intelligence, with perhaps a very deep interest in the subject—certainly men of fidelity, conscientiousness, and zeal without measure—but men who have not only no training in statistical methods and in statistical administration, but have also lacked that elementary knowledge of the subject which was necessary to save them from making great errors of judgement, and sometimes monstrous errors in their conclusions.
>
> I do not know of a single man now holding, or who has ever held, a position in this country as the head of a statistical bureau, or as chief of a statistical service, or as a statistician, who had any elementary training for his work. All those who have had anything to do with American statistics came into the service comparatively late in life, without any elementary training [52].

Many of the same ideas he had emphasized in his address at the opening session of the International Statistical Institute in Chicago, September 11, 1893 [8].

Thus, Walker was a strong advocate of a permanent bureau of the census. He was a pioneer in the use of colors in graphic presentations. He was, moreover, much in favor of more frequent use of graphic presentation instead of relying exclusively on statistical tables, as was the customary policy of handling statistics [34]. He strongly urged the training of statisticians in our colleges and universities. As the first president of the newly-formed American Economic Association (1885–1892) he played a strong part in its formative years and as the fourth president of The American Statistical Association (1883–1897) he was greatly instrumental in having college professors and government officials become members. He awakened not only the interest of Congress in better census taking but also the interest of the public in the value of such statistical information as was indicated by his articles in the *Atlantic Monthly, The*

Quarterly Journal of Economics and *The Forum*. Many institutions honored him; he received the Ph.D. from Amherst and Halle, and the LL.D. from Amherst, Columbia, Harvard, Yale, Dublin, Edinburgh and St. Andrews, and he was an honorary fellow of the Royal Statistical Society.

RICHMOND MAYO-SMITH (1854–1901)

Richmond Mayo-Smith, college professor, economist, editor, and statistician, the third of five children, born in Troy, Ohio, received his early education in the schools of Dayton, where his parents had moved in 1856 [13, Vol. 10, 237]. His father was a successful railway car manufacturer [9, Vol. 12, 467–468]. He graduated from Amherst College with an A.B. in 1875 and an A.M. in 1878, and later with the Ph.D. in 1890. One of Mayo-Smith's teachers, Professor John W. Burgess [4] influenced him to go to Europe for his postgraduate work; there he studied at the Universities of Berlin and Heidelberg during the period 1875–1877 [35, Vol. 29, 223–224]. Burgess, who had accepted a position at Columbia College, where he later founded the graduate School of Political Science in 1880 (changed to Faculty of Political Science in 1887), had promised Mayo-Smith a teaching position. Thus Mayo-Smith joined the staff in the fall of 1877 as an instructor in history and political science. From 1878 to 1883 he served as an assistant and adjunct professor of political economy and social science, and in 1883 he was made full professor. Moreover, he was one of the five original professors [20] of the newly-created graduate School of Political Science, and in 1890 when Columbia was reorganized, he was chosen one of the members of the council, a position he held until his death [40, 683–684]. He was, furthermore, one of the founders of the American Economic Association at Saratoga, N. Y., in 1885, and was very active in rebuilding the American Statistical Association, serving as one of its vice-presidents, 1897–1901, and was made an Honorary Fellow of the Royal Statistical Society in 1890. The same year he was elected a member of the National Academy of Sciences—a unique honor for a social scientist and statistician. He also served as one of the original board of editors of *Political Science Quarterly*, which started in 1886. He took an active part in the International Statistical Institute, attending meetings at Vienna (1891), Chicago (1893), St. Petersburg (1897) and Christiania (1899). Incidentally, Mayo-Smith's right name was Smith, and it is said that because there was another faculty member in the same school of Political Science with this name, Edmund Munroe Smith, he added his mother's maiden name, Mayo.

While Mayo-Smith did not teach the first course in statistics offered by any American university, as some claim, he was the first professor to teach the subject on a more scientific basis [15]. Indeed, he succeeded in doing so not only by his excellent classroom teaching but also by his many scholarly writings on statistics in the form of books, articles, and book reviews. He published in 1888 a 127-page monograph entitled *Statistics and Economics*, a work that many either overlook or confuse with a later volume of the same title. This work was issued as one of the publications of the American Economic Association (September and November 1888, published as one issue, Volume III, Numbers 4 and 5) and is considered the second American statistics text. His best known statistical writing is, of course, the two-volume work bearing the broad title *Science of Statistics*, consisting of Volume I, *Statistics and Sociology* (Macmillan, 1895) of 399 pages, and Volume II, *Statistics and Economics* (Macmillan, 1899) of 467 pages, which served as standard texts for many years. He also wrote a book *Emigration and Immigration* (Scribners, 1890) [54 (1901–1902), 765]. Besides, he served as chairman of a special committee, consisting of such distinguished members as Davis R. Dewey, Roland P. Falkner, Walter F. Willcox, and Carroll D. Wright, of the American Economic Association, which

studied ways and means of improving federal census work. From this resulted a 516-page volume entitled "The Federal Census: Critical Essays by Members of the American Economic Association," published in March 1899 by the Association in its *Publications* (New Series, No. 2).

He was also the first statistics professor to offer a statistical laboratory and seminar in any American university. This laboratory, actually listed as statistical laboratory and seminar, met for two hours fortnightly in the academic years, 1897–1901. He also wrote many articles and book reviews earnestly striving to place statistics on a higher scientific basis. A few of these articles are "American Labor Statistics," "On Census Methods," "The Study of Statistics," and "A Permanent Census Bureau," all appearing in the *Political Science Quarterly* (Volumes I, V, X, XI respectively). Another article was "Wage Statistics and the Next Census of the United States" in the *Quarterly Journal of Economics* (Volume II).

While he was a scholar of high rank in the field of economics, as indicated by his writings, Mayo-Smith was even more distinguished in the field of statistics, where he was without a peer. He was the most eminent American scientific statistician and one of the outstanding ones of the world in the late nineteenth century. Having come under the influence of the German Historical School of the 1880's, like such leading American scholars as Richard T. Ely, John B. Clark, Henry Carter Adams, Roland P. Falkner, and others, he was a strong advocate of the use of quantitative methods in placing social sciences in general, and economics and statistics in particular, on a sound scientific basis [42], [43]. Notwithstanding his pioneering efforts and energetic zeal, he was nevertheless well aware of the limitations of statistics. Furthermore, with General Francis A. Walker, president, and Carroll D. Wright, vice-president, he played an important part in strengthening the American Statistical Association and took considerable part in recommending improvements of the federal census.

CARROLL DAVIDSON WRIGHT (1840–1909)

Carroll Davidson Wright, administrator, college professor, military leader, social economist, and statistician, the third of seven children, was born at Dunbarton, N. H. He received his early education in public schools and academies at Washington, N. H., Chester, Vt., and Reading, Mass. While teaching in country schools, he studied law [1], [27], [9, Vol. 20, 544–545]. In September 1862 he enlisted as a private and rose to the rank of colonel by the end of the Civil War. He was admitted to the bar of New Hampshire in 1865 and of Massachusetts in 1867, taking up his residence in Reading, Mass. [13, Vol. 15, 499–500]. Twice he was elected to the Massachusetts Senate in 1871 and 1872, and he became an outstanding member. Because of the considerable ill will existing toward the Bureau of Statistics of Labor, established in 1869, Governor Washburn induced Wright to take charge in 1873, lest the legislature abolish it [35, Vol. 9, 7–8]. Wright wrote to Walker for advice on the proper administration of such a Bureau of Labor Statistics, and part of the reply is as follows:

> I have given much thought to the letter in which you do me the honor to ask me my views as to the work of the Massachusetts Bureau of Labor Statistics, . . . I feel . . . that the work can be done to the satisfaction of all citizens; and that your office has only to prove itself superior alike to partisan dictation and to the seductions of theory, in order to command the cordial support of the press and of the body of citizens. If any mistake is more likely than others to be committed in such a critical position, it is

> to undertake to recognize both parties as parties and to award so much in due turn to each. This course almost inevitably leads to jealousy and dissatisfaction. If any office is strong enough simply to consider the body of citizens, and to refuse to recognize or entertain consideration of parties, success is already in the main assured. . . . I have strong hopes that you will so distinctly and decisively disconnect the Massachusetts Bureau of Labor Statistics from politics,—from dependence on organizations, whether of working men or of employers, and from the support of economical theories, individual views or class interests—to command the moral support of the whole body of citizens, and receive the cooperation of all men of all occupations and degrees [48].

Wright proceeded to reorganize the bureau, and with great tact and impartiality finally succeeded in making this bureau a model for others to imitate. Later he was appointed the first Commissioner of the Bureau of Labor in the United States Department of Interior, created by an act of Congress on June 27, 1884 [37, Vol. 3, 809–811].

While Chief of the Massachusetts bureau, Wright also organized and supervised the Massachusetts Census of 1875, regarded as the finest and most comprehensive census work ever undertaken at that time. Thus, when superintendent of the 1880 federal census, Walker had Wright take charge of that part of the national census relating to Massachusetts. Wright also successfully supervised the Massachusetts State Census of 1885.

Examination of Wright's reports will reveal that he always sought the truth. It is perhaps partly a tribute to Wright that thirty-four states established bureaus of labor. Wright, upon taking office as United States Commissioner of Labor Statistics, gave some of his ideas in a letter to his superior, Lucius G. C. Lamar, Secretary of the Interior. In part he says:

> Declaring this, then, to be the positive policy of the Bureau of Labor, this office makes its initial work that of pure fact. . . . It should be remembered that a bureau of Labor cannot solve social or industrial problems, nor can it bring direct returns in a material way to the citizens of a country, but its works must be classed among educational efforts, and by *judicious investigations* and the *fearless publication* of the *results* thereof, it may and should enable the people to more clearly and more fully comprehend many of the problems which now vex them [60], [28].

His philosophy of investigation is indicated by one of his statements:

> If the investigator be really scientific in his methods of study, he cares not so much to be pleased by what the results may bring out, as to feel assured that the showing is accurate; he is ready at all times to recast his opinions, to modify his reasoning, and even turn his mind into new channels of thought, whenever the facts indicate that such changes should be made; his face is always turned to the light [63].

Wright not only firmly established the bureau in our national life but also was so influential in his contacts with the heads of the bureaus of labor of the several states that they, in turn, continued to elect him as head of their national organization, entitled "The National Association of Chiefs and Commissioners of Bureaus of Statistics of Labor in the United States," from 1885 until he retired in 1905 [37], [38, Vol. 3, 809–811].

Wright, furthermore, enjoyed the high esteem of both Republican and Democratic presidents (Arthur, Harrison, McKinley, Theodore Roosevelt, and Cleveland) and thus served during their administrations. Because of declining health, he retired from office January 31, 1905 to accept the presidency of Clark University on a full time basis; he had been serving as president since 1902 on a part-time basis.

Wright exerted a considerable influence in the academic world by his courses at Catholic University of America, Columbian College (now George Washington University), Harvard, Johns Hopkins, and Michigan, as well as by public lectures at Dartmouth, Vassar, and elsewhere. He strongly advocated the teaching of statistics in our colleges and universities. Addressing the joint session of the American Economic and Historical Associations on May 24, 1887, Wright stressed a number of important ideas. He opened this address by saying:

> America has no counterpart to the continental school of statisticians, whose members have entered their particular field of science after special training by a systematic course of instruction. We have our statisticians, to be sure, but they have taken up their work accidentally, and not as a profession. . . . Many of the leading colleges and universities of the continent make special effort to fit men to adopt statistical science as a branch of administration, or as a profession [58].

He described briefly the character of courses in statistics being offered at such leading universities as Leipzig, Tübingen, Würzburg, Breslau, Dorpat, Halle, Kiel, Königsberg, Munich, Strasburg, Prague, Vienna and others in contrast to the very few institutions in this country such as Johns Hopkins, Massachusetts Institute of Technology and Columbia [58].

He called attention to the fact that:

> Every well appointed college has its chair of political economy, and this department can be broadened sufficiently to take in statistical science, without impairing efficiency in this or any other department [58].

More forcefully he remarked that:

> We take the census in the United States every ten years, but as a rule the men that are brought into the work know nothing of statistics: they should be trained in the very elementary work of census-taking and of statistical science [58].

At another time, speaking before the American Statistical Association, May 9, 1890, Wright described that in the Italian Universities:

> not only is the science of statistics taught in the universities of Italy, but it is taught also in every one of the technical institutes of the country, about eighty in number. . . . It is an obligatory study for law students in the universities, and it must include the theory of statistics and the statistics of the kingdom [59].

His bureau became the training ground of a group of young promising statisticians and it excited the admiration of the statisticians of Europe. His ability as a statistical investigator is attested by the twenty-three volumes of the Massachusetts Bureau of Statistics of Labor and thirty-eight annual and special reports of the United States Bureau of Labor. He was well aware of the limitations of statistics. Horace Wadlin, Chief, Massachusetts Bureau of Statistics of Labor, 1888–1903, gives one example:

> His familiarity with figures has not bred a contempt but a profound reverence. No man knows better than he the value and the honesty of figures, and no man realizes more fully the grave responsibility which rests upon him who uses them. In his terse, lucid way of speech he is given to epigrammatic expression. He said once to a friend: 'Figures won't lie, but liars will figure.' And he said again 'It scares me to death to hear people use figures loosely.' These two expressions convey a very clear idea of the spirit with which he approaches statistics [48].

Mayo-Smith was critical of some of Wright's statistics, especially in the early days of the Massachusetts Bureau and of some of the United States Bureau of Labor material [30], [31]. Wright did not attempt to employ statistical techniques to draw inferences and generalizations. He was somewhat distrustful of statistical theory. He was well aware of the controversy raging at that time: "Is statistics a science or a method?" The former view held that statistics merely sought to reduce human behavior to laws, as advanced by Quetelet and others, while the latter view sought to describe more carefully the problem in quantitative form.

Wright, besides being president of the American Statistical Association (1897–1909) and also of the National Association of Chiefs and Commissioners of Bureaus of Statistics of Labor (1885–1905), was the recipient of honorary degrees: M.A., Tufts, Ph.D., Dartmouth, and LL.D., Wesleyan, Clark, Tufts, and Amherst. He was an honorary fellow of the Royal Statistical Society.

CHARLES F. PIDGIN (1844–1923)

Charles Felton Pidgin, author, inventor, and statistician, born in Roxbury, Mass., was an only child to survive infancy. Upon graduating from Boston English High School in 1863 with honors, earning the Franklin medal, he became a bookkeeper, and later a newspaper writer. In 1870 he entered the manufacturing business but after two years he severed his connections and returned to newspaper writing [9, Vol. 14, 573–574]. In 1873 he joined the Massachusetts Bureau of Statistics of Labor as its chief clerk which he was thus to serve in this capacity for thirty years (1873–1903) before he was appointed its fourth director for one term 1903–1907. He failed to obtain a reappointment [35, Vol. 13, 479–480], [40, 800].

Pidgin's long connection with the Massachusetts Bureau of Statistics of Labor gave him opportunities to display his inventive talent and ingenuity in devising methods and inventing various electrical and tabulating machines for the rapid and accurate mechanical computation of statistical data, and their graphic presentation. He was keenly aware of the urgent need of statistical and tabulatlng machines in order to expedite census taking and other statistical work. Some of his machines were adapted to the needs of this bureau while others were patented and developed on a commercial scale [54, Vol. 12, p. 2473].

He published his first book in 1888, a handbook of 201 pages entitled *Practical Statistics*, containing a sub-title "A Handbook for the Use of the Statistician at Work, Students in Colleges and Academies, Agents, Census Enumerators, etc.," which was well reviewed in September 1888 issue of the *Political Science Quarterly* by Professor Mayo-Smith of Columbia College, probably America's most outstanding professor of statistics in this century:

> The author . . . is perhaps the best practical statistician in America. He is the inventor of numerous arithmetical and calculating machines and devices for saving labor . . . the two volumes of the Massachusetts Census of 1885 which have been published give ample evidence of his skill and the comprehensive value of his methods. This volume gives the results of his experience and a full view of the interior workings of what is undoubtedly the best statistical office in America and which is probably not excelled in Europe. . . . It cannot fail to be of the utmost value to the heads of our statistical bureaus and to statisticians of our board of trade . . . who so often enter on their work without the slightest knowledge of statistical methods. . . . The book,

however, is intended for and deserves a far wider circle of readers than the comparatively limited number of practical statisticians. It should be read by every student of statistical science, by every teacher of political economy or of any of the social sciences where statistics are used [32].

He intended to publish a second work, *History of Statistics*, for he announced it on the copyright page of his first book. No reasons can be found for his failure to publish this history. Later, in 1896, he published another volume, *An Index to Occupations*, sub-titled "for the use in Census work and Statistical Tabulations," a work of 300 pages, which was the forerunner of our works dealing with the industrial classifications, an important statistical aid. This book pointed out 22 main kinds of occupation with 123 sections involving 17,253 classes. In 1876, as a contribution to the Centennial Exhibition in Philadelphia, he published a 102-page report entitled *History of the Bureau of Statistics of Labor of Massachusetts* with a sub-title "and of Labor Legislation in that State from 1838 to 1876" which became a part of the Seventh Annual Report of the bureau.

Pidgin has been praised for his work in the Massachusetts state censuses of 1875, 1885, and 1895, for which he prepared the schedules and supervised their successful completion.

He retired in 1907, almost blind from a cataract, and spent the remainder of his life dictating stories, novels, and poems, as well as words for musical comedies, operas and other writings, a number of which enjoyed a wide sale. Some of his writings were *Quincy Adams Sawyer* (1900), *The Further Adventures of Quincy Adams Sawyer* (1909), *Chronicles of Quincy Adams Sawyer, Detective* (1912), *The House of Shame* (1912), *The Courtin'*, a comic opera (1913), and the libretto of a grand opera, *Venusia and Cupidon* (1919). *The Electric Spark*, a comic opera, was produced more than 3000 times in the United States and England. He perfected a "visible speech" system for motion pictures (1917). Some of his musical publications were *Mercedes, Cambyses or the Pearl of Persia*, and *Sunflowers and Lillies*.

CONCLUSIONS

Several interesting features stand out. Of the seven leading American statisticians, four were college graduates, Jarvis, DeBow, Mayo-Smith and Walker, while three, Shattuck, Pidgin, and Wright, were not. Three had legal training, DeBow, Walker and Wright. Only one, Mayo-Smith, had academic training as a statistician. Mayo-Smith and Walker were the only ones who had post-graduate training. Three taught statistics, Mayo-Smith at Columbia, Walker at Yale, and Wright at the Catholic University of America, Columbian College (now George Washington University), Clark, Harvard, Johns Hopkins and Michigan. Three were superintendents of the federal census, DeBow in 1850, Walker in 1870 and 1880, and Wright in 1890, while Shattuck and Jarvis assisted in the preparation of the federal censuses, and Shattuck conducted the 1845 Boston Census.

The eminent position in nineteenth century statistics occupied by Massachusetts is brought out prominently by the careers of these seven. Five of them, Shattuck, Jarvis, Walker, Pidgin, and Wright were from the Bay State. Their

ideas, efforts, and zeal contributed tremendously to the advancement of statistics in the nineteenth century, which Sir Robert Giffen, K.C.B., LL.D., F.R.S., a famous English statistician, president of the Royal Statistical Society, 1882–1884, has described as the "statistical century par excellence."

Among promising young statisticians just embarking in the field at the end of the nineteenth century were Henry Carter Adams (1851–1921), professor of political economy at Michigan, president of the American Economic Association for two years (1896–1897), and chief statistician, United States Interstate Commerce Commission; Davis R. Dewey (1852–1942), president of the American Economic Association in 1909, who taught statistics at Massachusetts Institute of Technology for many years; Roland P. Falkner (1866–1940), probbably the first American professor to devote full time to teaching statistics, offering a wide variety of courses in statistics at the University of Pennsylvania beginning in 1888–1889; Irving Fisher (1867–1947), professor of political economy at Yale, president of the American Statistical Association in 1932, of the American Economic Association in 1918, and of the Econometric Society in 1931–1933, and author of many economic and statistical works and articles; John Koren (1861–1923), a statistician for many years with the United States Department of Labor, editor of the *History of Statistics* (1918), and president of the American Statistical Association for two years, 1913 and 1914; Henry Ludwell Moore (born 1869), professor of political economy at Columbia, author of many works and articles dealing with the application of statistical techniques to economics, and generally conceded to be the pioneer American econometrician; Warren M. Persons (1878–1937), president of the American Statistical Association in 1923 who was a pioneer in time series analysis; Walter F. Willcox (born 1861), one of America's most distinguished statisticians, who served several times with the United States Census, who attended a number of international statistical conferences, who was a prolific writer on statistical subjects, president of the American Statistical Association in 1912 and also of the American Economic Association in 1915. He wrote more than 130 papers for various publications, the first one appearing in the *Political Science Quarterly* in 1893, and the most recent in the *Journal of the American Statistical Association* in December, 1954, this covering what seems to be a record span of over sixty years. Perhaps it may be of some interest to indicate that certain other promising young scholars, beginning their teaching at the close of the nineteenth century, were pioneers in employing statistical techniques in their disciplines such as Franz Boas (1858–1942), famous anthropologist at Columbia, James McK. Cattell (1860–1944), and Edward L. Thorndike (1874–1949), both famous psychologists at Columbia.

Finally, the principal contribution to statistics by these seven leaders was their intense zeal in setting up adequate records to obtain accurate facts as well as the proper interpretation of them. Perhaps their keynote was that of Carroll D. Wright: "*judicious investigations and fearless publication.*" Thus we can well understand why very little statistical theory was developed by these seven American statisticians, although Professor Mayo-Smith was beginning to emphasize the importance of averages, dispersion, probability, correlation, and other statistical techniques toward the end of the 1890's. American statisticians

in this century had tremendous tasks to perform, at times, against obstacles, political or other. They had to find proper ways and means for the accurate collection, classification, and presentation of quantitative data, to perfect correct use of statistical tables and charts, and to find out how to interpret their data. They were pioneers blazing a trail, at times rather slowly, through a wilderness of imperfect material. Moreover, they had to advocate strongly the need for trained enumerators, to devise pertinent and adequate classifications of data, to develop better definitions of important statistical units, to secure uniformity and comparability of data, as well as to perform other statistical functions. As they were accomplishing these tasks, such famous English scholars as Francis Y. Edgeworth, Sir Francis Galton, Karl Pearson, G. Udny Yule, and others were by their writings pointing out newer statistical techniques to be employed in handling statistical data and formulating generalizations. The publication of Arthur L. Bowley's *Elements of Statistics* in 1901 played a tremendous part in the future use of these tools by American statisticians in the twentieth century. It appears, therefore, that statistics in the nineteenth century was "political arithmetic," a group of numerical facts attempting to describe given situations, and only in the twentieth century statistics did it come to include highly specialized techniques for the purpose of drawing inferences.

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STATISTICS AND SCIENCE*

E. J. G. PITMAN
University of Tasmania†

SIR RONALD FISHER indicates, in the introductory chapter to his new *Statistical Methods and Scientific Inference*, that the purpose of the book is the elucidation and consolidation of the specifically logical concepts which have emerged in the development of statistics.

In both of the closely related operations of statistical estimation and statistical testing we pass from observed values of random variables to the distributions of those random variables. We attempt to make statements, or to test statements, about more or less unknown distributions. That is the abstract mathematical formulation of the problem. From the practical point of view we may think of arguing from observed effects to their causes. Until this century the only method of dealing with such a problem was to calculate a conditional *a posteriori* distribution of causes (distributions) from a given or assumed *a priori* distribution. As a practical tool this method is hopelessly inadequate because it is so rarely applicable. Now we have significance tests, and confidence intervals and/or fiducial distributions. Discussion of these modern tools and of mathematical likelihood, Behrens' test, and the principles of estimation takes up most of Fisher's book.

SIGNIFICANCE TESTS

On this topic, Fisher writes as follows:

> The common tests of significance . . . have come in the first two quarters of the twentieth century to play a rather central part in statistical analysis. In the day-to-day work of experimental research in the natural sciences, they are constantly in use to distinguish real effects of importance to a research programme from such apparent effects as might have appeared in consequence of errors of random sampling, or of uncontrolled variability, of any sort, in the physical or biological material under examination. (p. 75)
>
> The logical basis of these scientific applications was the elementary one of excluding, at an assigned level of significance, hypotheses, or views of the causal background, which could only by a more or less implausible coincidence have led to what had been observed. (p. 4)
>
> In general tests of significance are based on *hypothetical* probabilities calculated from their null hypotheses. They do not generally lead to any probability statements about the real world, but to a rational and well-defined measure of reluctance to the acceptance of the hypotheses they test. (p. 44)
>
> Though recognizable as a psychological condition of reluctance, or resistance to the acceptance of a proposition, the feeling induced by a test of significance has an objective basis in that the probability statement on which it is based is a fact communicable to, and verifiable by, other rational minds. The level of significance in such cases fulfils the conditions of a measure of the rational grounds for the disbelief it engenders.

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* An invited review article on Sir Ronald A. Fisher, *Statistical Methods and Scientific Inference*, New York: Hafner Publishing Company, 1956. Pp. viii, 175. $3.00.

† This review article was written while the author was Visiting Professor in the Department of Statistics, Stanford University.

> It is more primitive, or elemental than, and does not justify, any exact probability statement about the proposition. (p. 43)
>
> The force with which such a conclusion is supported is logically that of the simple disjunction: *Either* an exceptionally rare chance has occurred, *or* the theory of random distribution is not true. (p. 39)

All this is a nice expression of personal feelings combined with a firm insistence on the objectivity of the probability statement upon which the test of significance is based. Opinions may differ somewhat about the feelings but we must agree about the probability statement.

It seems to me that the point of view indicated by the last three quotations is that of the scientist when he frames the hypothesis to be tested *after* looking at the observations. The reluctance to accept the hypothesis is engendered by the observations themselves. The first example he gives, Michell's calculations on the stars, is exactly a case of this kind. This treatment of hypotheses suggested by the observations is a proper scientific procedure, but it has special dangers *because* we may not be able to calculate any probability of being right or wrong, and it is not the most important use of statistical tests.

Often the reluctance to accept the hypothesis exists before the observations are obtained, these being the result of an experiment specially designed to test the hypothesis. Suppose that a simple hypothesis H is subjected to a significance test at significance level α, and that a significant result is obtained. A scientist who was already inclined to reject H may now feel that he may safely do so, because H has failed to pass the test, an unlikely event (probability α) if it were really true. Fisher's description of feelings does not fit this case.

Whatever his previous suspicions about H and whatever his feelings about such tests, the scientist knows that if he formulates the hypothesis H to be tested before he makes his observations, the probability that the test will give a significant result will be α if H is true. If he frequently tests hypotheses at this level (not necessarily sampling from the same population, or testing the same hypothesis, or using the same test), and if he rejects the hypothesis tested whenever a significant result is obtained, he may expect that in a fraction α *of those cases in which the hypothesis tested is true* he will reject it.

This is an expression of the risk of wrong rejection—of looking for a mare's nest—which a scientist incurs when he subjects a hypothesis to such a test. Fisher seems to misunderstand it. Immediately after the last quotation given above (p. 39) he goes on, "In view of the efforts which have been made to force a frequency interpretation on to such a disjunction . . .". No one forces a frequency interpretation on to the *disjunction:* the frequency interpretation has to do with the "rare chance."

> The attempts that have been made to explain the cogency of tests of significance in scientific research, by reference to hypothetical frequencies of possible statements, based on them, being right or wrong, thus seem to miss the essential nature of such tests. (p. 41)

The scientist with ideas frames his hypotheses and wishes to test them. He would be happy if he could devise a test which would decide with certainty whether a hypothesis is true or not. When he cannot do this, he must use a test which has only a certain probability of giving the right answer.

> To a practical man, also, who rejects a hypothesis, it is of course, a matter of indifference with what probability he might be led to accept the hypothesis falsely, for in his case he is not accepting it." (p. 42)

Perhaps, after the decision, this may be so; but when choosing the test he will use, the practical man should be interested in its liability to error.

FIDUCIAL DISTRIBUTIONS AND CONFIDENCE INTERVALS

The beginning of all this was Fisher's 1930 paper on "Inverse Probability" [1], from which the following two quotations are taken:

> In many cases the random sampling distribution of a statistic, T, calculable directly from the observations, is expressible solely in terms of a single parameter, of which T is the estimate found by the method of maximum likelihood. If T is a statistic of continuous variation, and P the probability that T should be less than any specified value, we have then a relation of the form
>
> $$P = F(T, \theta).$$
>
> If now we give to P any particular value such as .95, we have a relationship between the statistic T and the parameter θ, such that T is the 95 per cent value corresponding to a given θ, and this relationship implies the perfectly objective fact that in 5 per cent of samples T will exceed the 95 per cent value corresponding to the actual value of θ in the population from which it is drawn. To any value of T there will moreover be usually a particular value of θ to which it bears this relationship; we may call this the "fiducial 5 per cent value of θ" corresponding to a given T. If, as usually if not always happens, T increases with θ for all possible values, we may express the relationship by saying that the true value of θ will be less than the fiducial 5 per cent value corresponding to the observed value of T in exactly 5 trials in 100. By constructing a table of corresponding values, we may know as soon as T is calculated what is the fiducial 5 per cent value of θ, and that the true value of θ will be less than this value in just 5 per cent of trials. This then is a definite probability statement about the unknown parameter θ, which is true irrespective of any assumption as to its *a priori* distribution. [1]
>
> . . . the fiducial distribution of a parameter θ for a given statistic T may be expressed as
>
> $$df = -\frac{\partial}{\partial\theta} F(T, \theta)d\theta,$$
>
> while the distribution of the statistic for a given value of the parameter is
>
> $$df = \frac{\partial}{\partial T} F(T, \theta)dT. \quad [1].$$

The basic idea in this argument is extremely simple, but no one seems to have thought of it before Fisher. Given a number k, between 0 and 1, define $K(\theta)$ by

$$P\{T \leq K(\theta)\} = k. \tag{1}$$

If $K(\theta)$ is an increasing function of θ, the inequality $T \leq K(\theta)$ is equivalent to $\theta \geq K^{-1}(T)$ where K^{-1} is the inverse function of K. Thus (1) can be written

$$P\{\theta \geq K^{-1}(T)\} = k. \tag{2}$$

It is hardly possible for present day students of statistics to appreciate the liberating effect of the mathematically trivial step from (1) to (2), a simple twisting of an inequality. Neyman expressed himself as follows: [2]

> The possibility of solving the problems of statistical estimation independently from any knowledge of the *a priori* probability laws, discovered by R. A. Fisher, makes it superfluous to make any appeals to the Bayes theorem. [2]

> The solution of the problem which I described as the problem of confidence intervals has been sought by the greatest minds since the work of Bayes 150 years ago. Any recent book on the theory of probability includes large sections concerning this problem. These sections are crowded with all sorts of "paradoxes," etc. The present solution means, I think, not less than a revolution in *the theory* of statistics. [2]

If the statistic T has a continuous distribution function,

$$F(x, \theta) = P\{T \leq x\},$$

the random variable $F(T, \theta)$ has a continuous, uniform (rectangular) distribution from 0 to 1. Hence, if k is any fixed real number from 0 to 1,

$$P\{F(T, \theta) \leq k\} = k. \tag{3}$$

Suppose that the possible values of T form an open interval (b, c) and that the possible values of θ form an open interval (β, γ). Suppose further, what is often true, that $F(x, \theta)$ is a continuous, strictly decreasing function of θ, and that for $x \epsilon (b, c)$, $F(x, \theta) \to 1$ when $\theta \to \beta$ and $F(x, \theta) \to 0$ when $\theta \to \gamma$.

K is uniquely determined by $F(T, K) = k$. The inequality $F(T, \theta) \leq k$ is equivalent to $\theta \geq K$. Hence (3) may be written $P\{\theta \geq K\} = k = F(T, K)$. Therefore

$$P\{\theta \leq K\} = G(T, K), \tag{4}$$

where $G(T, K) = 1 - F(T, K)$. For any observed value of T, the function $G(T, K)$ is a distribution function in K, and

$$G(T, \beta) = 0, \qquad G(T, \gamma) = 1;$$

we may call it the fiducial distribution function of θ determined by the observed value of T. In Fisher's terminology K is the fiducial $100(1-k)$ per cent value of θ corresponding to T.

Formally, (4) resembles a statement about a random variable θ; but it must be remembered that K is not a fixed number. It is a specified quantile of the fiducial distribution function, for it is determined by $G(T, K) = 1 - k$, where k is some given fixed number. This K is a function of T, and so is a random variable. Thus (4) is really a statement about this random variable. By this twisting of inequalities we are able to make statements about the unknown parameter θ with a calculable probability of being right; but this does not give θ the status of a random variable as Fisher asserts:

> By contrast, the fiducial argument uses the observations only to change the logical status of the parameter from one in which nothing is known of it, and no probability statement about it can be made, to the status of a random variable having a well-defined distribution. (p. 51)

The restriction on K must always be borne in mind.

If k_1, k_2 are given numbers such that $0 \leq k_1 < k_2 \leq 1$, and if K_1, K_2 are respectively the k_1 quantile and the k_2 quantile of $G(T, K)$, i.e. if

$$G(T, K_1) = k_1, \qquad G(T, K_2) = k_2,$$

then it follows from (4) that

$$P\{K_1 \leq \theta \leq K_2\} = k_2 - k_1 = G(T, K_2) - G(T, K_1). \tag{5}$$

If $G(T, K)$ has an integrable derivative with respect to K, we may write

$$P\{K_1 \leq \theta \leq K_2\} = \int_{K_1}^{K_2} \frac{\partial G(T, K)}{\partial K}\, dK = \int_{K_1}^{K_2} \frac{\partial G(T, \theta)}{\partial \theta}\, d\theta.$$

The fiducial distribution of θ determined by T has then the frequency function $\partial G(T, \theta)/\partial\theta = -\partial F(T, \theta)/\partial\theta$, as Fisher indicated in the quotation above.

Neyman [2] extended (5) to the case where k_1, k_2 are not necessarily constants but may be functions of θ such that $k_2 - k_1$ is constant. He called (K_1, K_2) a *confidence interval* and the constant $k_2 - k_1$ the *confidence coefficient*. He preferred this terminology because he feared misunderstandings about the meanings of fiducial probability and fiducial distribution. His fears have been justified. Up to this point the theory of fiducial probability and the theory of confidence intervals are essentially the same. The confidence interval terminology focusses attention on the real content of statements, the fiducial distribution is an elegant way of exhibiting all confidence intervals.

Neyman's treatment is applicable to cases where $F(x, \theta)$ is not monotonic in θ, and it can be extended to discontinuous distributions of T; but in the latter case exact probability statements are no longer possible, only statements of inequalities. Fisher's remarks on confidence limits in this book are very misleading.[1]

> It has been frequently stated, as though it were a characteristic property of confidence limits, that the interval between them will in repeated samples cover the true value with the exact frequency corresponding with the level of significance chosen. E.g. that in 98% of trials the true value would be found to lie between the two 1% points. This, if true, would give them the force of a statement of probability. However, actually, the true value will lie between the assigned limits generally in more than 98% of such trials, and no exact statement of probability can be inferred. Exactly verifiable probability statements are not a characteristic of confidence limits, as they are of the limits that can be assigned when the fiducial argument is available (pp. 65, 66).

Exactly verifiable probability statements *are* a characteristic of confidence limits when the distribution of T is continuous.

THE FIDUCIAL DISTRIBUTION OF SEVERAL PARAMETERS

Fisher considers a random variable with a probability density

$$\frac{1}{\beta} \exp\left\{\phi\left(\frac{x - \alpha}{\beta}\right)\right\},$$

in which ϕ is a function of known form, which may be taken to be differentiable almost everywhere, and α and β are two parameters specifying the location and scale of the distribution. There are N observed values $x_1, x_2, \cdots, x_N$. He finally says:

> . . . in fact the simultaneous distribution of α and β in the light of a given sample is
>
> $$df \propto \exp\left[S\left\{\phi\left(\frac{A - \alpha}{\beta} + u\,\frac{B}{\beta}\right)\right\}\right] \frac{B^{N-1}}{\beta^{N+1}}\, d\alpha d\beta,$$
>
> for the actual set of u-values observed." (p. 161)

[1] They should be compared with his reported remarks in the discussion on Neyman's paper [2].

Sorting out the meanings of A, B, u, we find that this may be expressed in the form

$$df \propto \exp\left[\sum \phi\left(\frac{x_r - \alpha}{\beta}\right)\right]\frac{B^{N-1}}{\beta^{N+1}}\,d\alpha d\beta,$$

where B is a constant for a given sample. In other words, the fiducial distribution of α, β determined by the sample has a probability density or frequency function

$$\frac{c}{\beta^{N+1}}\exp\left[\sum \phi\left(\frac{x_r - \alpha}{\beta}\right)\right]. \qquad -\infty < \alpha < \infty, 0 < \beta < \infty \qquad (6)$$

where c is a constant. What does this really mean? Fisher does not explain. I gave a rigorous proof of this result in 1939 [3] and explained its precise meaning. Briefly, this is as follows. Denote the frequency function (6) by $g(\alpha, \beta)$. If D is a region in the (α, β) plane such that

$$\int_D g(\alpha, \beta)d\alpha d\beta = k,$$

and *if D satisfies certain other conditions*, then

$$P\{(\alpha, \beta)\epsilon D\} = k,$$

i.e. the probability that D will contain the true parameter point (α, β) is k.

D will satisfy the other conditions referred to if it is a region bounded by curves whose equations are of the form

$$\psi\left(\frac{x_1 - \alpha}{\beta}, \frac{x_2 - \alpha}{\beta}, \cdots, \frac{x_N - \alpha}{\beta}\right) = 0,$$

where ψ is a function which is continuous almost everywhere. This enables us to obtain the fiducial distributions of α alone and of β alone, as was explained [3]; but it does not enable us to obtain the fiducial distribution of *any* given function of α and β as Fisher seems to think. Replying to a criticism of Bartlett's about the treatment of location and scale parameters of a normal distribution, where α and β are replaced by μ and σ, Fisher says,

> His statement that "there is no reason to suppose that from it we may infer the fiducial distribution of, say, $\mu+\sigma$" is presumably due to some analytic misapprehension. The problem involves no more than ordinary integration over the known bivariate frequency distribution, and its solution had been published before Bartlett wrote. (p. 162)

The problem does involve more than ordinary integration over the known bivariate frequency distribution, because the region of integration must satisfy certain conditions, as I have indicated. For this reason I doubt the validity of the unspecified solution mentioned. It is a pity that no proper reference to it was given.

THE BEHRENS TEST

Disregard of the restrictions on the region of integration makes all "proofs" of the Behrens test, offered here or elsewhere, fallacious. That there is something wrong with the mathematical argument is evident from the fact that it reaches no exact probability statement which is true. Fisher admits the latter but fails to realize that it implies the former:

> This circumstance, indeed, caused me no surprise, . . . but was eagerly seized upon by M. S. Bartlett, as though it were a defect in the test of significance of a composite hypothesis, that in special cases the criterion of rejection is less frequently attained by chance than in others (p. 96).

What the circumstance indicates is a defect in the "proof." There is no mention of inequalities in the argument. Fisher is quite definite about the exactness of conclusions reached by the fiducial argument:

> Probabilities obtained by a fiducial argument are objectively verifiable in exactly the same sense as are the probabilities assigned in games of chance (p. 59). Exactly verifiable probability statements are not a characteristic of confidence limits, *as they are of the limits that can be assigned when the fiducial argument is available.* (pp. 65, 66, italics mine)

Let α and β be two unknown parameters. Suppose that α has a fiducial distribution with density function $g(\alpha)$ and distribution function $G(\alpha)$. This means that if k is any given number between 0 and 1, and if K is defined by $G(K) = k$, then $P\{\alpha \leq K\} = k$. Suppose also that β has a fiducial distribution with density function $h(\beta)$ and distribution function $H(\beta)$, and that the two fiducial distributions are independent, i.e. that they are determined by independent statistics.

Define K by

$$\int_{\alpha-\beta\leq K} g(\alpha)h(\beta)d\alpha d\beta = k.$$

It is not in general true that

$$P\{\alpha - \beta \leq K\} = k. \tag{7}$$

We cannot simply integrate over an arbitrary region in the (α, β) plane and obtain a probability statement. If D is a region in the plane bounded by curves with equations of the form

$$\psi\{G(\alpha), H(\beta)\} = 0,$$

and if

$$\int_D g(\alpha)h(\beta)d\alpha d\beta = k,$$

then it is true that

$$P\{(\alpha, \beta)\epsilon D\} = k;$$

but the region $\alpha - \beta \leq K$ will in general not be of this type. The Behrens test is obtained by using such an unjustifiable integration.

Although (7) is not in general true, it is reasonable in some cases to expect that it may be approximately true, or that it may be true if the equality is replaced by an inequality, especially when k is near to 0 or to 1. For the Behrens test applied to two samples of two, Fisher has obtained an inequality. For other cases no such inequality seems to have been obtained, and so the exact size (level of significance) of the test is unknown. Fisher says nothing about this:

THE LIKELIHOOD FUNCTION

Let

$$x = (x_1, x_2, \cdots, x_n)$$

be a set of observed values of random variables with a distribution which has a density function $\phi(x, \theta)$ depending on a parameter θ. For given values of $x_1, x_2, \cdots$, the function $\phi(x, \theta)$ regarded as a function of θ is called the likelihood function determined by the sample. Fisher has often emphasized its prime importance in the estimation of θ:

> The Method of Maximum Likelihood is indeed much used and widely appreciated in the statistical literature, without, I fancy, so much appreciation of the significance of the system of likelihood values at other possible values of the parameter. In the theory of estimation it has appeared that the whole of the information supplied by a sample is comprised in the likelihood, as a function known for all possible values of the parameter. (p. 69)

The Neyman-Pearson theorem tells us that if θ can take only two values θ_1, θ_2, then there is a most powerful test of given size α for testing the hypothesis that θ has one value against the alternative that it has the other value, and that this test is based on the likelihood ratio $\phi(x, \theta_2)/\phi(x, \theta_1)$. It seems not to be generally realized that in this simple case the likelihood ratio is a sufficient statistic. This is perhaps most easily seen by writing

$$\phi(x, \theta) \equiv \phi(x, \theta_1)\left[\frac{\phi(x, \theta_2)}{\phi(x, \theta_1)}\right]^{(\theta-\theta_1)/(\theta_2-\theta_1)}$$

$$\equiv \phi_1(x)\phi_2(T, \theta),$$

where $T = \phi(x, \theta_2)/\phi(x, \theta_1)$, $\phi_1(x)$ is a function of x only, and $\phi_2(T, \theta)$ a function of T and θ only. Thus T is a sufficient statistic; when it is fixed, the distribution of any other statistic is independent of the value of θ, which can only be θ_1 or θ_2. Similarly, if θ can take only m values

$$\theta_1, \theta_2, \cdots, \theta_m,$$

the set of likelihood ratios

$$\phi(x, \theta_1) : \phi(x, \theta_2) : \cdots : \phi(x, \theta_m)$$

is a sufficient statistic for the estimation of θ.

Hence, in choosing among any finite subset of θ values, anything supplied by the sample except the likelihood ratios is irrelevant. In this sense the whole of the information supplied by the sample is comprised in the likelihood function, as Fisher says. But it must be remembered that there is something that is not

supplied by the sample, that is the function ϕ. Its values at points other than the sample point affect probabilities, and so the likelihood function does not tell the whole story.

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NONPARAMETRIC STATISTICS*

I. Richard Savage
University of Minnesota†

0. INTRODUCTION

Statistical procedures whose validity does not depend on the underlying random variables having a special form, such as Gaussian, are known as distribution-free or nonparametric. The amount of published material on nonparametric statistics is increasing rapidly. A bibliography published five years ago listed 999 entries [37], and perhaps another 500 have appeared since. The use of nonparametric techniques has become widespread, and they appear now to be entering introductory statistics textbooks. Specialized courses in nonparametric inference are given in several universities, and meetings of statistics professional associations usually include sessions, and almost invariably include papers, on the subject.

Both applied and theoretical statisticians have felt a need to have this material brought together. In recent months Fraser [12] and Siegel have provided books aimed to help fill this need.[1] Siegel's book is designed for the research worker with a limited mathematical and statistical background, Fraser's for the advanced student of mathematical statistics. Thus, practice and theory are divided between the two books much as they are divided between chapters in Kendall's rank correlation book [19].

Nonparametric methods are needed in many fields, and can be applied in all. Siegel, by his choice of sub-title and examples, has directed his book to the attention of behavioral scientists. As will be brought out later, however, little behavioral science is involved in it, and the interest of the book for research workers will be the same whether they are interested in behavioral science or not.

Siegel's book contains nine chapters and twenty-one tables. The first three chapters review principles of testing hypotheses, the theory of scaling, and the advantages and disadvantages of nonparametric methods. Other chapters treat (4) one sample, (5) two matched samples, (6) two independent samples, (7) k matched samples, (8) k independent samples, and (9) multivariate samples. Each of these chapters offers several alternative techniques. For each technique, the discussion is organized in a standard pattern, covering the intuitive appeal of the technique, the distribution of the test statistic under the null hypothesis (separately for small and large samples), and completely worked examples. This organization is a natural one and makes it easy to find specific material.

In spite of the need for a book on nonparametric statistics for the research worker, I cannot recommend this one. The conception is good, but the execution

* An invited review of Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*. New York: McGraw-Hill Book Company, 1956. Pp. xvii, 312. $6.50.

† This article was written while the author was at the Center for Advanced Study in the Behavioral Sciences.

[1] A review of Fraser's book appears elsewhere in this issue.

is bad. The bases for these judgments, together with some constructive suggestions, can be summarized under the following four headings, around which the rest of this article is organized:

(1) *Scope.* The only form of statistical inference considered is testing hypotheses of "no difference." Neither other types of hypotheses nor estimation is considered. Furthermore, the exposition of the principles of testing "no difference" hypotheses is frequently faulty; in particular, the treatment of power is never adequate.

(2) *Organization.* There is frequent, undesirable repetition. Many of the procedures are introduced several times with different names and in slightly different contexts. Usually the fact of repetition is not made clear. No effort is made to show how the procedures can be generalized or used in new situations. The dual treatment of large and small samples is unnecessary. Frequently-occurring topics, such as ties, limit theorems, and randomization to achieve a desired level of significance, are not treated in a unified manner.

(3) *Behavioral Science.* Articulation with behavioral science is shallow. The examples are sketchy. The relation of statistical techniques to the substance of research is not properly presented.

(4) *Alternative Sources.* There are readily available collections of material on nonparametric statistics that are comparable in scope and sounder in presentation.

1. SCOPE

1a. *Forms of Inference.* While tests of significance have dominated the literature of nonparametric inference, there have been other important developments, for example, tolerance intervals and confidence intervals. The recent elementary text of Wallis and Roberts,[2] for example, explains how confidence intervals can be generated from tests and applies this in the nonparametric case [48, pp. 461–3, 593–8]. Nonparametric test statistics frequently constitute estimates of important parameters: the sign test statistic estimates the probability of a positive response; the Wilcoxon two sample test statistic (rather, a known function of it) estimates the probability that an individual selected at random from one population will have a larger score than an individual selected at random from a second population; the Kendall rank correlation statistic estimates the difference between the probabilities of concordance and discordance.

Siegel does not explain why his interest is confined to tests of significance; to make measurements and then ignore their magnitudes would ordinarily be pointless. Exclusive reliance on tests of significance obscures the fact that statistical significance does not imply substantive significance.

The tests given by Siegel apply only to null hypotheses of "no difference." In research, however, null hypotheses of the form "Population A has a median at least five units *larger* than the median of Population B" arise. Null hypotheses of no difference are usually known to be false before the data are collected

[2] The Wallis-Roberts book was published only about four months before Siegel's, so presumably was not available to him.

[9, p. 42; 48, pp. 384–8]; when they are, their rejection or acceptance simply reflects the size of the sample and the power of the test, and is not a contribution to science.

1b. *Influence of scale of measurement in choosing a test.* The question of what test statistic may be used appropriately on particular sets of data is repeatedly mishandled. The erroneous statement is frequently made that arithmetic operations should not be performed if the results are not meaningful in terms of the phenomenon the data measure. (See pp. 21–30 and sections marked "ii. Statistical Test" in each example.) This implies that I.Q. scores should never be added, since two I.Q.'s of 50 are not like one of 100 (on p. 139, however, "aggression scores" are added). I know of no reason to limit statistical procedures to those involving arithmetic operations consistent with the scale properties of the observed quantities. Thus, in working with I.Q. scores, the Fisher-Pitman randomization test is certainly acceptable even if the t test is not. The randomization test has the desired level of significance, and for many alternatives has good power.

As a matter of fact, it is not always clear which operations are involved in particular test procedures; different computational procedures, one acceptable by Siegel's criterion and another unacceptable, may be logically equivalent and lead to identical results. The Wilcoxon signed-rank test and the Walsh test, for example, are essentially the same [44]. In the Wilcoxon test the observations are ranked from smallest to largest in absolute value, but in the Walsh test some of the observations are added together. Similarly, the Wilcoxon two-sample test and the Mann-Whitney test are equivalent; but the Mann-Whitney test is based only on comparisons of pairs of observations, while the Wilcoxon test requires the ranking of the observations from smallest to largest.

It is probably true, however, that sophisticated assumptions about the distribution of the observations are commonly unsupportable in situations where very few arithmetic operations with the data lead to numbers that are meaningful in terms of the phenomena under study.

1c. *Role of Normal Distribution.* Almost invariably, Siegel compares nonparametric procedures with the corresponding normal procedures based on t, χ^2, or F. (He never states explicitly the basis of the comparison; see sections marked "Power and Power-Efficiency" in the treatment of each procedure.) The value of nonparametric techniques lies in the fact that they may be used without making the assumption of normality. Thus, in order to evaluate these procedures their operating characteristics should be found for the largest possible class of distributions. Whenever possible, one should go further than just considering normal situations. Siegel's exclusive interest in normal alternatives weakens the book by excluding consideration in any detail of the power functions of the sign test, various tests related to contingency tables, and tests of randomness. This weakness is illustrated by: "Inasmuch as there is no parametric technique applicable to data measured in a nominal scale, it would be meaningless to inquire about the power-efficiency of the binomial test when used with nominal data" (p. 42). If the binomial test is the only procedure available, then, by definition, its power efficiency must be one.

In spite of strong interest in normal alternatives, the author never gives power functions—not even for normal alternatives[3]—,never describes such frequently used tests for normality as the χ^2 goodness of fit test with parameters estimated from the data, and never supplies any data as to the effects of non-normality on tests involving normality assumptions.

1d. *Power Efficiency.* "Power efficiency" is defined as the ratio of the sample sizes required by two test procedures in order to obtain the same power. In order to use this concept, however, modifications and details are necessary. The meaning of a power efficiency of .95 when one of the sample sizes is 6 (see pp. 42, 67, etc.) is not covered by this definition, since, for example, $.95 \times 6$ and $6/.95$ are not integers. Some notion of a fractional observation must be introduced. Also, the power efficiency depends on the alternative that is being investigated and the significance level. Usually, when *asymptotic* efficiency is meant, this is made clear; but we read: "The randomization test for matched pairs, because it uses all of the information in the sample, has a power-efficiency of 100 per cent" (p. 92; see also p. 156). This statement should be limited to the asymptotic case and to comparing the randomization procedure to the appropriate t test. The meaning of "uses all of the information" is vague.

1e. *Miscellaneous Remarks on Principles of Inference.* Following is a small sample intended to illustrate the nature of the erroneous, imprecise, or ambiguous assertions which permeate the book:

"Roughly speaking, the larger χ^2 is, the more likely it is that the observed frequencies did not come from the population on which the null hypothesis is based" (p. 43). The use of "likely" suggests an *a priori* distribution which is never mentioned. Although this line of reasoning is interesting, it runs counter to the formal theory of testing hypotheses adopted as the framework of this book.

"Notice that the p we found by the Kolmogorov-Smirnov test is smaller than .01, while that found by the χ^2 test is larger than .05. This difference gives some indication of the superior power of the Kolmogorov-Smirnov test" (p. 51). If the null hypothesis is true, no such statement is true.

"That is, if the tables which assume that $F_0(X)$ is continuous are used to test a hypothesis about a discontinuous variable, the test is a conservative one: if H_0 is rejected by that test we can have real confidence in that decision" (p. 59). What should be said is that if the procedure is used where ties can occur, then the level of significance is less than that shown in the table. "Real confidence" adds nothing.

At one point, the null and alternative hypotheses are formulated in terms of the test statistic instead of the underlying populations (p. 78).

A statement is made (p. 126) to the effect that a nonparametric procedure (in this case the Wilcoxon test) can be superior to its "parametric alternative." What is meant is that for some types of underlying distributions it is better (in the sense of more powerful) to use the Wilcoxon statistic than it is to use the t test. However, there is always a parametric procedure at least as good as the

[3] The only example of a power function is on page 10, where a curve is given purporting to be the power function of the two-sided test for the mean of a normal distribution with known variance. The drawing is, however, grossly inaccurate, and serves only to show the most general features of the shape.

Wilcoxon procedure if only because, in any parametric context, the Wilcoxon procedure can be regarded as parametric. (With regard to this point, see the recent paper by Hodges and Lehman [18].)

In discussing multiple tests of significance, the computations are made as if the tests were mutually independent (pp. 159–60). This is not the case in considering all contrasts in the analysis of variance. An additional reference to Scheffé [39] would have been helpful at this point.

In applying the Kruskal-Wallis rank analysis of variance to the weights of pigs at birth the null hypothesis is rejected (p. 192). The data involve litters of various sizes. It is concluded that the rejection of the null hypothesis implies that the birth weights vary on the litter sizes. A more justifiable conclusion is that the litters have different average birth weights. When examining the same data Kruskal and Wallis [21] avoid this temptation to specify the cause for rejecting the null hypothesis and Snedecor [42, 4th ed., p. 238], from whom the data originate, explicitly warns the reader against it.

"The Spearman r_s and the Kendall τ are equally powerful in rejecting H_0, inasmuch as they make equivalent use of the information in the data" (p. 223). Actually, the Spearman and Kendall procedures for small samples do not have the same power. The statement applies only approximately for large samples. The expression "equivalent use" is not defined.

2. ORGANIZATION

Wisely, there is no attempt to cover all nonparametric procedures, but, unwisely, the existence of other procedures is not indicated, except at one place (p. 194). Frequently several procedures are introduced where an elementary and enlightening argument would show them to be equivalent. The following section contains a list of these equivalent procedures, and the section after that discusses the value of showing the equivalences.

2a. *Multiple Appearance of Procedures.* As has been pointed out, both the one sample signed-rank Wilcoxon test and the Walsh median test are included and treated separately when they are essentially the same. The material on the Walsh test adds five unnecessary (and misleading) pages. Also included are both the Friedman analysis of variance and the Kendall-Smith coefficient of concordance, although they are equivalent. The description of the Kendall measure takes eleven pages. The sign test is introduced several times: as the binomial test on page 36, as the McNemar test on page 63, and as the sign test on page 68. The use of chi-square for $r \times k$ contingency tables is duplicated unnecessarily: one treatment discusses two independent samples (p. 104), the other, k independent samples (p. 175). Two tests based on runs are introduced. They are the total number of runs above and below the median and the Wald-Wolfowitz total number of runs for the two-sample problem: under the null hypothesis these have the same distributions.

It is true that each time a test is introduced it is in a different setting and slightly different aspects of it are considered. Because the book may be used as a handbook some repetition is perhaps desirable, or at least unavoidable. But to omit any unified treatments, to repeat the details each time, and to make such switches as that between the Friedman and Kendall-Smith forms of the

Friedman test, is to introduce needless confusion and render the book almost useless as a textbook.

Part of the duplication can perhaps be explained, but not excused, by noting that several of the procedures have been published several times by authors who did not know of the earlier work or did not understand its relation to their own. That Wilcoxon's name should appear only once, and then only in the references, in the section on the Wilcoxon two-sample procedure is, however, scarcely explainable. (Incidentally, on p. 120 of this discussion, Wilcoxon's formula for computing the test criterion appears with no mention of Wilcoxon.) Another historical point is the neglect of the Wallis paper [46] in preference to the simultaneous paper of Kendall and Smith [20] when talking about the correlation ratio using ranks. If more attention had been paid either to the Wallis paper or to Friedman's later paper [14], the relation between the Friedman and the Kendall-Smith procedures would have been exploited.

2b. *Flexibility of Nonparametric Procedures.* The duplicate presentation of procedures and techniques causes not only excessive size and "cook-bookishness," but—more important—it keeps the book from conveying any appreciation of the flexibility of nonparametric procedures. Flexibility is well illustrated by the sign test. There are many reasons for the popularity of this test: the assumptions underlying the test are relatively weak; it is easy to compute the test statistic; the distribution of the test statistic is available for both the null and alternative hypotheses when working with small or large samples; and for many testing situations the test has fairly good power when compared to other tests available. These reasons all contribute, but the real popularity of the sign test comes from its usefulness in many and diverse experimental situations. It can be used whenever a dichotomized variable is being studied, such as diseased or not diseased, successful or not successful, male or female, change from good to bad or from bad to good. Often experimental data can be put in dichotomized form even if they did not originate in that form. Applying the sign test to numerical data, we replace the numbers by the two classes above and below the hypothetical median. When some frequencies on a scale with several values are small, it may be desirable to reduce the scale to two well-represented points. If, for example, on the four-point scale "like very much," "like," "dislike," and "dislike very much," the two extremes are seldom used, the scale can be replaced by the dichotomy "like" and "dislike." Thus, a large variety of experimental data can be cast into a form where the sign test is applicable. The sign test is applicable to other situations involving dichotomies even if the hypothesis does not involve a fifty-fifty split. Examples of other than fifty-fifty splits arise in genetics and in multiple-response questions with only one correct answer. In many such cases, estimation of the underlying parameter of the dichotomy is important. Then such tools as the Clopper and Pearson [6] charts for confidence intervals, not covered by Siegel, are useful. Finally, the sign test involves working with binomial variables and is in that sense "parametric." Hence, the use of sequential procedures and decision theory is possible. (Sequential nonparametric procedures, such as presented by Romani [36], are not covered in Siegel's book.)

Not all nonparametric procedures are as flexible as the sign test. The double dichotomy based on two natural classifications, however, is the same as the Brown and Mood [5] model for testing that two populations have the same median; and it is natural to generalize it to testing that two populations have the same lower quartile or other centile. The reason the total number of runs in two samples and the total number of runs above and below the median have the same distribution under the null hypothesis can be seen as follows: In considering runs above and below the median, form two samples, the first containing the serial numbers corresponding to observations below the median, and the second composed of the serial numbers corresponding to observations above the median. The serial numbers occurring in the two samples are not observations on mutually independent random variables but the dependence is "symmetric." Under these circumstances, the procedure may be applied to the two samples of serial numbers. Since we already have tables of the distribution of runs for the case of unequal sample sizes, it is desirable to point out that there are analogous procedures to runs above and below the sample median, such as runs above and below the hypothetical median, or runs above and below the sample quartile. By this time the reader would be curious about the possibilities of using runs when he has more than two kinds of elements and a brief treatment of runs of several kinds of elements would be desirable [28], [47], [48]. It would also help the reader avoid confusion to tell him that there is another useful type of run, runs up and down [24, 30]. The Fisher-Pitman randomization technique [34] is a general procedure and should be treated as such. Several uses are made of it, but it is not pointed out that it is always usable, and, in particular, it is neglected in the chapter on correlation.

2c. *Special Problems.* The discussion of each procedure includes several short paragraphs according to a standard outline. The sections on large samples are almost the same as those for small samples, except for a different table of significance levels. Both tables could easily be included in the same discussion, and using the same example would serve to compare the large- and small-sample techniques. Although there is usually a paragraph on power efficiency—that is, the comparative power of a test relative to some other—this is never related to the examples; and there is no discussion of the absolute power of a test—for example, whether a particular experiment was designed so that interesting results could reasonably be expected with samples of the sizes used. The separation in the examples of the "statistical test," "sampling distribution," and "region of rejection" gives rise to several steps with almost no separate content. It destroys the idea of a test of significance as a decision procedure.

Certain problems recur frequently, yet there is no place where they are treated in a unified manner. Thus, there is no mention of the particular types of central limit theorems useful in nonparametric work (see Fraser [12] Chap. 6). There is no unified treatment of tied observations.[4] The only discussion of ancillary randomization to attain a specified level of significance from an otherwise discontinuous test is for the 2×2 table (p. 102). Such a discussion is

[4] The most extensive available discussion of ties is by Putter [35]. This covers the sign test and the Wilcoxon one-sample signed-rank test.

needed for the first example in the book, where a test at the .01 level is proposed and a test at the .0032 level is used. There is no general discussion of continuity corrections, although they are introduced several times.

3. BEHAVIORAL SCIENCE

The function of the examples in this book is to give detailed computations showing the steps in applying the procedures. Computational techniques such as checks and shortcuts, however, are not discussed. The examples do not give evidence as to which of the procedures have been found particularly useful in applications. The discussion surrounding the examples fails to suggest the role of statistical techniques in the completed research project. The emphasis is always on making isolated conclusions from a limited part of the data.

3a. *Depth of Examples.* The vocabulary of certain behavioral sciences is used superficially in some of the examples, but no specialized knowledge of the subject matter is required to understand them. Most of the examples are unrelated to each other so that neither the subject matter nor the interrelations of statistical methods in different aspects of a research study are developed between examples. Within an example, the analysis related to the subject is not carried to a sufficient depth to give any real understanding of even the specific application of a statistical test. Thus, in the cross-cultural example begun on page 112 (the data are from Whiting and Child [49]), a "random sample of cultures" is required. The specification of the population of cultures and the method of selecting a sample are not discussed. This is, in fact, an extremely difficult task that has not been satisfactorily handled in any cross-cultural research, but until it is, tests of significance remain meaningless.

The data in the example begun on page 39 are treated as if they arose from one sample. Actually they came from two samples, the second set of observations having been collected after a significant result was obtained on the first set [1]. If two samples are combined when it is known that one of them is significant, the second test of significance is invalid, since there is a higher probability of rejecting the null hypothesis (when it is true) than the nominal level of significance indicates. How to combine the information of the two samples in an experiment of this kind must depend on the particular situation. One possibility is to use the first sample for exploratory purposes, i.e., find interesting hypotheses from it, and estimate the sample size required for testing these hypotheses. Then the experimenter can use the second sample for making inferences.

In the example begun on p. 54 the data used in making a test of randomness are only a portion of the relevant data in the source [40]. The experiment involved observing a group of children in pairs at different times. Of primary interest is the order in which the children appear, i.e., the first child, the second child, etc. In the example, the method of assigning orders to pairs of children observed simultaneously is not indicated. The children are observed twice, occasionally with the second observation on a particular child before the first on another child. Only the data from the first appearances are used in the example. The analysis of the example is formally valid, in that the significance levels are actually those claimed, but it would not satisfy a sensible experimenter, because half of the data are neglected.

3b. *Applications of Nonparametric Procedures.* That nonparametric procedures have a role special to the behavioral sciences is, I believe, false. They are important for the behavioral scientist, but most of them have been developed and used in other fields, for example, engineering, production, cloud seeding, medicine, and agriculture. On the other hand, if a particular technique has not been used and is of no theoretical interest, it should not be included in a book on applications.[5] Actually, it is not easy to find applications of all of the techniques in one substantive field. However, Siegel has failed to use extensive data on the use of nonparametric procedures which have been collected by Savage [37], Blum and Fattu [3], and Bradley and Duncan [4].

Several of Siegel's examples are artificial or trivial. This is true of the examples of the one-sample Kolmogorov-Smirnov test (p. 49) and the runs above and below the median test (p. 56). Goodman [16] has presented a good example of the Kolmogorov-Smirnov test and Wallis [47] and Wolfowitz [53] have discussed several interesting applications of the runs test. The example for the one sample chi-square goodness of fit test involves horse racing, despite the fact that χ^2 is the most frequently used nonparametric procedure in the behavioral sciences [3].

A striking illustration of the use of nonparametric statistics in research, specifically in experimental psychology, appears in Volume 45 (1953) of the *Journal of Experimental Psychology.* Of the 70 papers in this volume, at least 12 involve applications of nonparametric procedures, not counting chi-square tests of goodness-of-fit or contingency-tables. Presumably the procedures are being used still more now.

4. ALTERNATIVE SOURCES

The books and articles discussed in this section are alternative presentations of nonparametric statistics designed for an audience of the same statistical maturity as that of Siegel's book.

4a. *Textbooks.* From all that has been said already, it might still be supposed that Siegel has collected more nonparametric material than appears elsewhere. This is not in fact the case. The book by Walker and Lev [45], which includes a chapter by Moses on nonparametric procedures, covers nearly everything contained in the Siegel book (and, of course, it contains much more in the general field of statistics). Topics in the Siegel book which are omitted in the Walker-Lev text are the Fisher-Pitman randomization procedures, Kendall's tau, Cochran's Q test, and the Moses test. Omission of the randomization procedures would be a serious shortcoming in a theoretical treatise, but in practice these procedures are rarely used because of computational difficulties; their chief role in theory is to justify the use of "normal theory" even in nonnormal situations.[6] Walker and Lev present tables or approximate distributions for each procedure.

The Walker and Lev book has more in common with Siegel than others I examined. Dixon and Massey [7] include several nonparametric procedures

[5] The Wald-Wolfowitz test of total number of runs is a case in point. Siegel's example (pp. 138–141) for illustrating this procedure originally did not use it. For the limitations of the procedure see Mood [29] and Lehmann [23]. Siegel himself implies that the test should not be used when he writes: "The runs test also guards against all kinds of differences, but it is not as powerful as the Kolmogorov-Smirnov test" (p. 158).

[6] Randomization procedures were first introduced by R. A. Fisher [8, sec. 21] for this very purpose.

with the necessary tables for applying them. Also included in their book are many procedures based on order statistics. These procedures, although parametric in that they depend on the assumption of normality, are related to common nonparametric procedures in being easy to apply. Wallis and Roberts [48] consider at least one "deep" example (sec. 2.8.4) involving nonparametric techniques, include nonparametric confidence intervals, and present the Goodman and Kruskal [17] rational analysis of contingency tables.

4b. *Articles.* Many of the theoretical papers in nonparametric inference use a surprisingly small amount of mathematics and are accessible to the applied statistician. The survey articles by Smith [41],[7] Moses [31], and Blum and Fattu [3], can be used as basic reading in nonparametric inference; for many of the techniques, these papers contain sufficient information to make applications without finding other references. For many of the techniques, expository material has appeared which is particularly helpful when detailed information is required. In particular there are source papers by Friedman [13] and by Kruskal and Wallis [27] on the nonparametric analysis of variance. In Kendall's book on rank correlation the mathematics and applied material are well separated. The writings of Wilcoxon [50, 51] are most pleasant even for the beginning student. The technical material on the Kolmogorov-Smirnov procedures is difficult, but there are good expositions by Goodman [16], Massey [26], Birnbaum [2], and Miller [27].

In conclusion, Siegel's book stands as the first attempt to give a full dress presentation of nonparametric procedures in a form suitable for the research worker. The material presented is arranged so that it may be easily found. All other aspects of the book are open to severe criticism.[8]

APPENDIX

The following minor comments may help readers of the book, and will help substantiate my evaluation of it. All comments regarding the tables are presented together, regardless of where the tables are discussed in the text.

A1. *Text*

P. 8. The references suggested for reading in decision theory will be too advanced for most users of this book. Alternatives are Girshick [15], L. J. Savage [38], Luce and Raiffa [25], and Williams [52].

P. 8. "Associated probability" is used here and defined on p. 11 as the probability of an event at least as "extreme" as the one observed. The meaning of "extreme" is not made clear for the two-sided case.

P. 13. The statement that "the region of rejection is a region of the sampling distribution" can be understood by noting that the "sampling distribution" is defined by Siegel as the possible values of the test statistic, i.e., the sample space. His "sampling distribution" is not a distribution.

P. 15. In the formula for binomial probabilities, "P" is the probability of "success" and "Q" is the probability of "failure." It should be noted that $Q = 1 - P$. Here and on p. 71 Siegel is being irrelevantly general since he is interested only in the case $P = Q = \frac{1}{2}$.

P. 20. When the assumptions underlying a test are not met, not only the level of significance, but also the power, will be difficult to find.

P. 21. The material on measurement covers only the univariate case, and thus does not apply to the chapter on correlation.

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[7] Coombs has an article on scaling in the same reference.

[8] Professor Siegel's comments on a draft of this article resulted in the removal of several errors. This does not imply that he agrees with this article in any way.

P. 32. "2. If sample sizes as small as $N=6$ are used, there is no alternative to using a nonparametric statistical test unless the nature of the population distribution is *known exactly.*" It would be desirable to present data regarding the effects of non-normality. Departure from normality will, no doubt, disturb the significance levels of normal tests more for small samples then for large samples. On the other hand, with larger samples it is frequently desirable to work with smaller error rates; hence the comparison of small and large samples is not straightforward. The basis of "$N=6$" is unknown to me. For very small samples there is a special difficulty with nonparametric procedures, since their distributions are discrete. Thus, using the two-sided sign test with a sample of four, we must either have a significance level at least as large as $\frac{1}{8}$ or else use a randomized test. If a test at the ten per cent level is desired we would reject only if all of the signs are the same *and* we had a success with a randomization device with a success probability of .8.

P. 44. The "observations" referred to in computing the degrees of freedom are the observed frequencies in the various classes. This applies to p. 106, also.

P. 49. The example for the one-sample Kolmogorov-Smirnov test involves a discrete hypothetical distribution; thus the level of significance will be less than assumed. This is discussed on p. 59.

P. 55. For an even number of observations, "middlemost score" can be interpreted as the mean of the two middle observations.

P. 67. The difference in results between the binomial and chi-square tests depends not only on the coarseness of the chi-square tabulation but also on the fact that the chi-square test is an approximation. On p. 74 the difference is due only to coarseness.

P. 74. In Table 5.6, "rank with less frequent sign" should read "smaller sum of like-signed ranks."

P. 83. In a symmetrical distribution, the probabilities associated with negative deviations from the median are the same as the probabilities associated with corresponding positive deviations. The symmetry assumption is required for the Wilcoxon test as well as for the Walsh test.

P. 95. The examples described as "random sampling" actually involve "systematic sampling." Thus, in selecting every tenth house from a list of 1,000, there are only 10 possible samples. The usual interpretation of "random sampling" would be sampling without replacement, i.e., each of the $\binom{1000}{100}$ possible subsets of 100 from the original 1,000 would have the same probability of being the sample. If corner houses all have addresses ending with the same digit (or if, as is in fact common, about a quarter of them have addresses ending in one digit and a quarter another digit), a systematic sample will either heavily under- or over-represent corner houses.

P. 113. The "sampling distribution" can be stated before the experiment. The expected cell frequencies are known because all of the marginal totals are made equal in the Brown-Mood procedure.

P. 126. In case C, where $n_2 \geq 20$, the normal approximation cannot be used if n_1 is small. Fix and Hodges [11] have considered this possibility.

P. 128. The two sample Kolmogorov-Smirnov test is introduced here for discrete distributions, but it derives its nonparametric interest from the continuous case where exact significance levels are obtained.

P. 131. The occurrence of a chi-square variable in finding levels of significance of the one-sided Kolmogorov-Smirnov test is an interesting mathematical fact, but not related to the uses of chi-square that have occurred previously.

P. 137. The word "sign" is used here in the sense of "symbol," not plus or minus.

P. 172. Table 7.5 indicates that for the examples considered the two test procedures frequently give the same result. This, however, does not give information about the power since we are not told whether the null or alternative hypotheses were true.

P. 182. In Table 8.2 the headings presumably should be "8 or less," "9 or 10," and "11 or 12."

P. 201. The measure C is not directly comparable to the other measures of correlation in that they do not measure the same thing. In fact, all of the measures of correlation differ in what they measure and hence none of them are directly comparable.

P. 232. The lower limit of Rs_{av} is $-1/(k-1)$, not -1.

A2. *Tables*

All statistical tables are listed, whether or not I have comments, in order to describe the contents.

Table A (Normal distribution). The blanks in the lower right portion of the table are not explained, but actually indicate that the appropriate values do not differ appreciably from those given in the margin.

Table B (Critical value of t). It should have been explained that the sequence of the last five values for df facilitates interpolation [7].

Table C (Chi-square critical values). It should have been explained that if N (the df) >30 critical values may be approximated, for example by

$$\chi_p^2 = \tfrac{1}{2}(\sqrt{2N-1} + U_p)^2$$

where U_p is the corresponding one-sided critical value for a normal distribution.

Table D (Binomial distribution, $P=Q=\frac{1}{2}$). The blanks in the lower left portion indicate that the values are almost zero. The †'s in upper line indicate that the values are exactly 1.

Table E (One-sample Kolmogorov-Smirnov critical value). It is not made clear that this is a one-sided (upper-tail) table and the levels of significance are exact.

Table F (Critical value of runs). These tables are symmetric in n_1 and n_2 so that the values above the diagonals are redundant. In F_I the probability of getting not more runs than indicated is at most .025 and the probability of getting more than the number of runs indicated is more than .025. Thus the instructions in the heading are wrong for the Wald-Wolfowitz test. The level of significance that will be used if the instructions are followed will be .025 or slightly less, not .05 as claimed. The Dixon and Massey [7] runs table is similar to this one. (In their instructions on page 326 (1st edition), for the table corresponding to F_I, change "not more" to "at least." This is corrected in the 2nd edition.)

Table G (Wilcoxon one-sample critical value). This table was originally adapted from a report by Tukey [44]. The levels of significance obtained by using the indicated values are as close as possible to the stated level of significance. Exact levels are not obtained for the test statistic, since it has a discrete distribution. The actual levels are sometimes above and sometimes below the stated values.

Table H (Walsh test critical value). Some of the exact probabilities here can be used immediately in finding the corresponding exact probabilities in Table C.

Table I (Contingency tables critical value). These are one-tailed probabilities and err on the "conservative" side as pointed out on page 99.

Table J (Two-sample Wilcoxon distribution). Values are given until the probabilities have accumulated to one half. (See the footnote on page 117).

Table K (Wilcoxon two-sample critical value). The entries in this table appear to be on the "conservative" side, i.e., the actual level of significance is not larger than the stated level of significance. The lower left could be left blank by symmetry. In Table K_{IV}, n_1 and n_2 are interchanged.

Table L (Two-sample Kolmogorov-Smirnov critical value). This table is for the case of two samples of equal size. Notice that for several sample sizes the one- and two-sided tests have the same critical values. Thus it "costs" no more to use a two-sided test in those cases. Clearly, some of the suggested values are on the safe side, but I have not checked all of them.

Table M (Two-sample Kolmogorov-Smirnov critical value).

Table N (Friedman analysis-of-variance distribution).

Table O (Kruskal-Wallis analysis-of-variance distribution).

Table P (Spearman's rank correlation critical value). The levels of significance appear to be on the "conservative" side. It should be noted that in the Olds [32, 33] articles the quantity whose distribution is given is $\sum d^2$, which is easier to compute than the rank correlation and is just as informative if only a test of significance is desired.

Table Q (Distribution of Kendall's tau).

Table R (Critical value of Kendall's measure of concordance). Entries in Table R may be obtained from those in Table N by multiplying those in N by $mn\,[n+1]/12$ where n is the number of treatments and m is the number of judges. The roles of k and N are interchanged in Table N and R.

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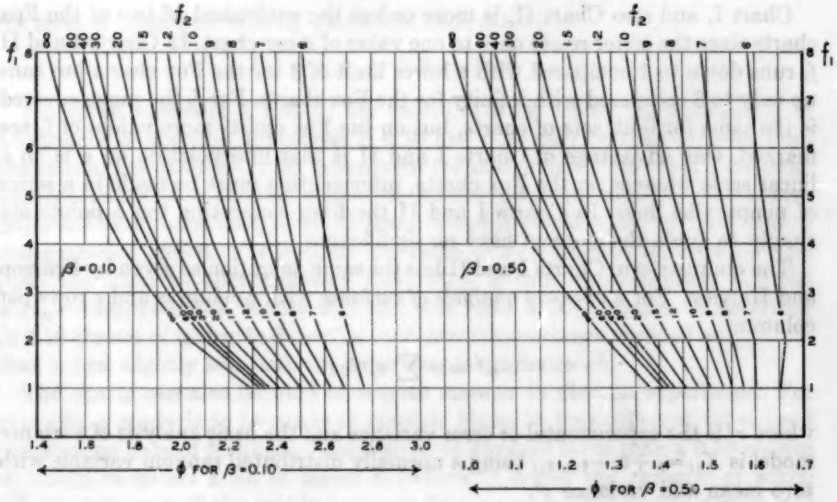
CHARTS OF THE 10% AND 50% POINTS OF THE OPERATING CHARACTERISTIC CURVES FOR FIXED EFFECTS ANALYSIS OF VARIANCE F TESTS, $\alpha = 0.01$ AND 0.05

ACHESON J. DUNCAN
The Johns Hopkins University

TABLES of the power functions for fixed effects analysis of variance F tests have been published by Tang [4] and Lehmer [2] and charts of these power functions have been presented by Pearson and Hartley [3] and Fox [1]. Pearson and Hartley offer eight charts of power curves for $\alpha = 0.01$ and $\alpha = 0.05$ and degrees of freedom f_1 from 1 to 8 and f_2 from 6 to ∞. They cover eight pages, one for each value of f_1. Fox gives lines of constant ϕ (defined below) for $P = 0.5$, 0.7, 0.8 and 0.9 and $\alpha = 0.01$ and 0.05, and values of f_1 from 3 to ∞ and values of f_2 from 4 to ∞. Single charts are for a specified value of α and P.

CHART I

THE 10% AND 50% POINTS OF THE OPERATING CHARACTERISTIC CURVES OF FIXED EFFECTS ANALYSIS OF VARIANCE F TESTS, $\alpha = 0.05$



The charts presented here offer essentially a special condensation of the Pearson and Hartley charts. For $\alpha = 0.05$ Chart I gives values of ϕ for[1] $\beta = 0.10$ and 0.50 and various values of f_1 and f_2. Chart II does the same for $\alpha = 0.01$. This condensation facilitates use of the charts for the specified values of β and cheapens duplication for classroom or laboratory use or text publication.

[1] β is the ordinate of the operating characteristic curve and is the probability of accepting the null hypothesis; P is the ordinate of the power curve and is the probability of rejecting that hypothesis. Thus $\beta = 1 - P$.

CHART II

THE 10% AND 50% POINTS OF THE OPERATING CHARACTERISTIC CURVES OF FIXED EFFECTS ANALYSIS OF VARIANCE F TESTS, $\alpha = 0.01$

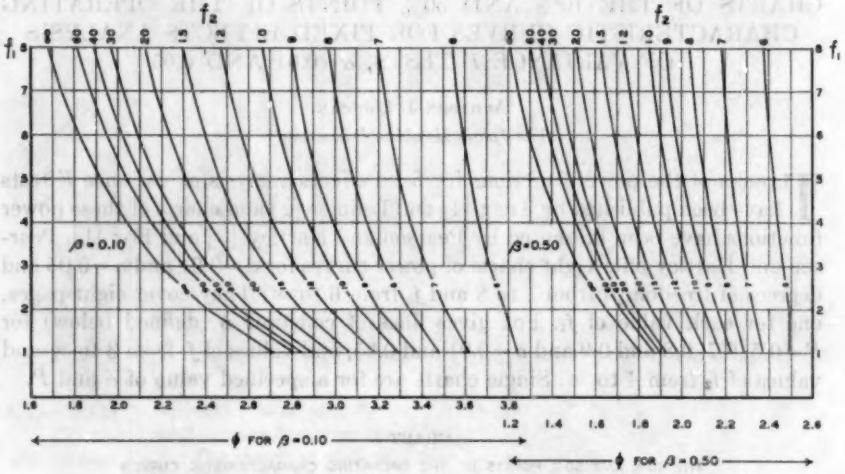


Chart I, and also Chart II, is more or less the equivalent of two of the Fox charts since the latter relate only to one value of β per chart. In Charts I and II f_1 runs down to 1 compared with a lower limit of 3 for the Fox charts but runs up only to 8 compared with infinity for the Fox charts. For f_2 the range covered is the same for both sets of charts, but on the Fox charts more values of f_2 are marked. One advantage of Charts I and II is that interpolation on ϕ is on a linear scale whereas on the Fox charts, interpolation must be made in a series of nonparallel lines. In Charts I and II the lines connecting the ϕ points are simply to guide the eye and have no significance.

The quantity ϕ in Charts I and II has the same definition as given by Pearson and Hartley. For a *one-way analysis of variance* with c columns and r rows per column,

$$\phi^2 = r \sum_j \theta_j^2 / c\sigma^2$$

where σ^2 is the experimental or error variance and the basic analysis of variance model is $X_{ij} = \mu + \theta_j + \epsilon_{ij}$, ϵ_{ij} being a normally distributed random variable with zero mean and variance σ^2.

For a *two-way analysis of variance with only one case in each cell,*

$$\phi^2 = r \sum_j \theta_j^2 / c\sigma^2$$

for tests of column effects ($H_0: \theta_1 = \theta_2 = \cdots = \theta_c = 0$); and

$$\phi^2 = c \sum_i \tau_i^2 / r\sigma^2$$

for tests of row effects ($H_0: \tau_1 = \tau_2 = \cdots \tau_r = 0$), where the basic model is

$$X_{ij} = \mu + \tau_i + \theta_j + \epsilon_{ij}.$$

For a *two-way analysis of variance with g cases in each cell,*

$$\phi^2 = rg \sum \theta_j^2 / c\sigma^2$$

for tests of column effects;

$$\phi^2 = cg \sum \tau_i^2 / r\sigma^2$$

for tests of row effects; and

$$\phi^2 = \frac{g}{(r-1)(c-1)+1} \sum_i \sum_j \gamma_{ij}^2 / \sigma^2$$

for tests of interaction effects, where the basic model is

$$X_{ijk} = \mu + \tau_i + \theta_j + \gamma_{ij} + \epsilon_{ijk}.$$

For a *Latin square*, $r \times r$,

$$\phi^2 = \sum_k \delta_k^2 / \sigma^2$$

for tests of treatment effects, where the basic model is

$$X_{ij} = \mu + \tau_i + \theta_j + \delta_k + \epsilon_{ijk}.$$

For given values of f_1 and f_2, the values of ϕ for which the probability of acceptance of the null hypothesis is 0.10 and 0.50 may be read directly from the charts. For example, in an experiment for analyzing the effects of 5 different concentrations of a solvent combined with 4 different catalysts, each with duplicate determinations, f_1 wouid equal 4 and f_2 would equal 20 for a test of concentration effects. For $\alpha = 0.05$ the value of $\phi_{0.10}$ for this test can be read from the chart as approximately 1.97. $\phi^2_{0.10}$ is thus approximately 3.9 and $\phi^2_{0.10}/r = 3.9/4 = 0.975$. Hence this test will have a 90% chance of detecting (a 0.10 chance of failing to detect) a concentration variance (defined as $\sum \theta_j^2/c$) that is just slightly less than the experimental variance σ^2.

The charts can also be used in reverse manner to plan an experiment. For example, a study is to be made of possible biases in four different tubes used in sampling bags of fertilizer. It is desired to have no more than a 0.10 chance of failing to detect a set of biases θ_j (where $\sum \theta_j = 0$) the variance of which ($\sum \theta_j^2/c$) is one half the within bag sampling variance. For $\alpha = 0.05$ this means that the number of samples to be taken by each tube must be such that

$$\phi^2_{0.10} = r(0.5), \text{ or}$$

$$\phi_{0.10} = \sqrt{0.5r}$$

This is an equation that can be solved by trial and error with the help of Chart I.

For $f_2 = \infty$, and $f_1 = 3$, Chart I yields $\phi_{0.10} = 1.88$. The value $(1.88)^2/0.5 \doteq 7$ is therefore a good value of r with which to start. With $r = 7$, $\phi_{0.10} = \sqrt{3.5} = 1.87$. But from Chart I, with $f_1 = 3$ and $f_2 = 24$, $\phi_{0.10}$ is approximately 2.05. Hence our r is not large enough. We next try $r = 10$. The ϕ becomes $\sqrt{5} = 2.24$. But from Chart I with $f_1 = 3$ and $f_2 = 36$, $\phi_{0.10}$ is approximately 2. Hence r is now too large. Some value between 7 and 10 is the desired answer. We try $r = 8$. Then ϕ becomes $\sqrt{4} = 2.00$, while Chart I with $f_1 = 3$, $f_2 = 28$, gives $\phi_{0.10}$ as approximately equal to 2.03. For $r = 9$, $\phi = \sqrt{4.5} = 2.12$, while Chart I with $f_1 = 3$, $f_2 = 32$, gives $\phi_{0.10}$ equal approximately to 2.00. It appears that $r = 8$ comes close to the desired result.

It will be noted that having f_1 and f_2 on the same chart facilitates computations which involve both these elements. For instance, suppose that in the previous example there was a rough limit (say 30) on the total number of samples that could be tested in the laboratory for this experiment. The question might then arise whether we should test 4 tubes with 7 samples per tube or 3 tubes with 10 samples per tube. From Chart I we see that with 4 tubes and 7 samples per tube, $\phi_{0.10}$ is 2.05 and

$$\frac{\sum \theta_j^2}{c\sigma^2} = \frac{(2.05)^2}{7} = 0.73.$$

For 3 tubes and 10 samples per tube $\phi_{.10}$ is 2.18 and

$$\frac{\sum \theta_j^2}{c\sigma^2} = \frac{(2.18)^2}{10} = 0.47.$$

In other words, we can in this instance, for slightly greater expenditure of time and money, detect variations between three tubes much more readily than we can detect an equal amount of variation between four tubes.

The 50% points of the operating characteristic curves are also given in Charts I and II to permit a rough interpolation of intermediate values of β and to allow designs of tests on an "indifference" basis.

APPENDIX

In Charts I and II values of β for $f_2 = 6$, 8, 10, 12, 20, 30, 40 and 60 have been based on numerical data provided by the National Bureau of Standards. Values of β for $f_2 = 7$, 9, 15 and ∞ have been read from the Pearson and Hartley charts and in most cases checked by the Fox charts. For $\alpha = 0.01$ and $\beta = 0.10$ the Fox chart has an upward bias for $f_2 = 7$ and values of β in Chart II for this value of f_2 were based solely on the Pearson and Hartley charts.[2] The error in reading ϕ from Charts I and II should be no more than ± 0.02.

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A PROBLEM IN LIFE TESTING

D. J. BARTHOLOMEW
Scientific Department, National Coal Board, London

1. INTRODUCTION

THE work contained in this paper arose out of the following practical problem:

Records are available giving the dates of installation of n items of equipment. Some of these have failed and the remainder are still in use. Using the dates of installation and the lives of those that have failed, we require an estimate of the average life.

Such a problem arises when the experimenter does not have complete control over the life test. First of all, he cannot always install all items at the same time, particularly if they have to be tested without interfering unduly with the normal running of a factory process. Secondly, he may be given a time limit for the experiment and so have to give an answer without reference to the state of the experiment at the end of that time.

2. THE LIKELIHOOD EQUATION

Let the time that has elapsed since the ith item was installed be T_i and let the life of this item be t_i. The life will only be known in those cases for which $t_i \leq T_i$. Any given sample will therefore consist of the quantities $T_1, T_2, \cdots, T_n$ and a certain number of completed lives. It seems desirable to emphasize the conditions which we have imposed on the problem. The sample size, n, is fixed in advance and the time at which the estimate is required is determined without reference to the experiment. Those items which fail are not replaced by new ones. Some work has been done for the case when different conditions are imposed, but this is not included in this paper. We assume that each item has the same life distribution with density function $p(t)$. The probability that the item installed a time T_i ago has been withdrawn to date is then

$$P_i = \int_0^{T_i} p(t)dt.$$

We shall write $Q_i = 1 - P_i$.

The estimate will be obtained by the method of maximum likelihood. The likelihood of a given sample can be written

$$l = \prod_{i=1}^{n} P_i^{a_i} Q_i^{1-a_i} \{p(t_i)/P_i\}^{a_i} \tag{1}$$

where

$a_i = 1$ if the item installed T_i ago has failed.
$= 0$ if the item installed T_i ago has not failed.

$\sum_1^n a_i = k$ is the total number of failures. k is therefore a random variable.

Taking the logarithm of the likelihood and simplifying we find

$$L = \log l = \sum_1^n \{(1 - a_i) \log Q_i + a_i \log p(t_i)\}. \tag{2}$$

3. THE EXPONENTIAL FAILURE DISTRIBUTION

3.1 The exponential failure distribution has been discussed by a number of writers. Davis [1] has given a number of examples of the occurrence of the distribution and Epstein and Sobel [2, 3] have given methods of obtaining the average life. It is frequently met in practice when the main cause of failure is some such random occurrence as damage.

For this distribution an explicit solution can be obtained from the likelihood equation. If we put

$$p(t) = \frac{1}{\theta} e^{-t/\theta} \qquad 0 \leq t < \infty, \theta > 0.$$

the average life to be estimated is θ. In this case L becomes

$$L = -\sum_1^n \{(1 - a_i)T_i/\theta + a_i(\log \theta + t_i/\theta)\}. \tag{3}$$

Setting $\partial L/\partial\theta = 0$ we have

$$\sum_1^n \{(1 - a_i)T_i/\theta^2 - a_i/\theta + a_i t_i/\theta^2\} = 0.$$

The value of θ satisfying this equation which maximizes the likelihood is

$$\hat{\theta} = \frac{1}{k} \sum_1^n \{a_i t_i + (1 - a_i)T_i\}. \tag{4}$$

Expressed in words, we add up all the lives to date, complete and incomplete, and divide by the number of completed lives. The estimate is only defined if at least one item has failed. To overcome the mathematical difficulties attendant on the case $k = 0$ we define

$$\hat{\theta} = \sum_1^n T_i$$

for $k = 0$. This amounts to assuming that one item fails at the point in time at which the estimate is made. In practice such an estimate would have little value.

The variance of the maximum likelihood estimate for large n has been given by R. A. Fisher (see for example Kendall [5]), who showed that

$$\text{var } (\hat{\theta}) = -1/E\left(\frac{\partial^2 L}{\partial \theta^2}\right).$$

Differentiating equation (3) twice we find

$$\frac{\partial^2 L}{\partial \theta^2} = \frac{k}{\theta^2} - \frac{2k\hat{\theta}}{\theta^3}.$$

In Sec. 3.2 we show that

$$E(k, \hat{\theta}) = \theta \sum_1^n P_i.$$

Also, k is the result of binomial sampling with unequal probabilities, $P_i (i = 1, 2, \cdots n)$, giving $E(k) = \sum P_i$. Using these results we have

$$E\left(\frac{\partial^2 L}{\partial \theta^2}\right) = \sum_1^n P_i/\theta^2$$

hence, in large samples

$$\text{var } (\hat{\theta}) = \theta^2 \Big/ \sum_1^n P_i. \tag{5}$$

Comparing this variance with that for the estimate of θ based on simple sampling, it follows that our estimate is about as good as if the number of observations in simple sampling had been equal to $\sum P_i$. As the true θ is unknown, the limiting variance may be estimated by $\hat{\theta}^2 / \sum \hat{P}_i$, where the $\hat{P}_i$'s are calculated using $\hat{\theta}$ instead of θ. As $E(k) = \sum P_i$, a quick estimate would be $\hat{\theta}^2/k$.

3.2 It is well known that the maximum likelihood estimate is often biased in small samples. In this section the bias of $\hat{\theta}$ is investigated. We use the result that

$$\text{cov } (k, \hat{\theta}) = E(k, \hat{\theta}) - E(k) \cdot E(\hat{\theta})$$

hence

$$E(\hat{\theta}) = E(k, \hat{\theta})/E(k) - \text{cov } (k, \hat{\theta})/E(k).$$

Now $k\hat{\theta}$ may be regarded as the sum of n variates the ith of which comes from the population

$$p(x_i) = \frac{1}{\theta} e^{-x_i/\theta}, \qquad 0 \leq x_i < T_i$$

and probability of $e^{-T_i/\theta}$ at $x_i = T_i$. The expected value of the ith member of the sum is readily shown to be

$$(\theta P_i - Q_i T_i) + Q_i T_i;$$

therefore, $E(k, \hat{\theta}) = \theta \sum P_i$. Using the fact that $E(k) = \sum P_i$ we find

$$E(\hat{\theta}) = \theta - \text{cov } (k, \hat{\theta}) \Big/ \sum_1^n P_i. \tag{6}$$

The bias of our estimate is therefore $|\operatorname{cov}(k, \hat{\theta})/\sum P_i|$. Now

$$|\operatorname{cov}(k, \hat{\theta})| \leq \{\operatorname{var}(k) \cdot \operatorname{var}(\hat{\theta})\}^{1/2}$$
$$= \left\{\sum_1^n P_iQ_i\right\}^{1/2} . \leq .E.(\hat{\theta}).$$

We can say, therefore, that

$$|E(\hat{\theta}) - \theta| \leq \left\{\sum_1^n P_iQ_i\right\}^{1/2} . \leq .E(\hat{\theta}) \Big/ \sum_1^n P_i.$$

Noting that $\sum P_iQ_i \leq \sum P_i$ and substituting the limiting value of the standard error of θ we have the weaker result,

$$|E(\hat{\theta}) - \theta| \leq S.E.(\hat{\theta}) \Big/ \left\{\sum_1^n P_i\right\} \sim \theta \Big/ \sum_1^n P_i. \tag{7}$$

It can be seen intuitively that $\hat{\theta}$ and k will be negatively correlated. The covariance of k and $\hat{\theta}$ will therefore be negative and hence $\hat{\theta}$ will tend to overestimate θ.

In the special case when $T_i = T$ for all i the problem is closely related to that treated by Epstein and Sobel [2]. The observed lives then become the shortest k out of n with the restriction that they must all be less than T. Our estimate then takes the form

$$\hat{\theta} = \frac{1}{k}\sum_1^k t_i + \left(\frac{n-k}{k}\right)T.$$

In Epstein and Sobel's problem, k is fixed in advance but there is no restriction on the range of values the t_i can take. Their estimate of θ is

$$\hat{\theta}' = \frac{1}{k}\sum_1^k t_i + \left(\frac{n-k}{k}\right)t_k$$

which is distributed exactly as $\theta\chi^2/2k$ with $2k$ degrees of freedom. When all the T's are equal the expected value of $\hat{\theta}$ can be found from the relation

$$E(\hat{\theta}) = \sum_k P(k)E(\hat{\theta}|k).$$

From this we find

$$E(\hat{\theta}) = \theta - QT/P - T + nTE(k^{-1}). \tag{8}$$

For large n

$$E(k^{-1}) = (1 + Q/nP + O(n^{-2}))/nP$$

Now $T = -\log Q$ so that

$$E(\hat{\theta}) = \theta(1 - Q\log Q/nP^2 + O(n^{-2})). \tag{9}$$

If exact values of $E(\hat{\theta})$ are required, the tables given by Grab and Savage [4] may be used for $E(k^{-1})$.

4. AN EXAMPLE

The following data illustrate the form in which the problem arises and the way in which the estimate is made.

TABLE 1

RESULTS OF A LIFE TEST ON TEN PIECES OF EQUIPMENT

Item No.	1	2	3	4	5	6	7	8	9	10
Date of Installation	11 June	21 June	22 June	2 July	21 July	31 July	31 July	1 Aug.	2 Aug.	10 Aug.
Date of Failure	13 June	—	12 Aug.	—	23 Aug.	27 Aug.	14 Aug.	25 Aug.	6 Aug.	—
Life in Days (t_i)	2	(119)	51	(77)	33	27	14	24	4	(37)
No. Days to End of Period (T_i)	81	72	70	60	41	31	31	30	29	21

The estimate is required on 31 August. The three lives in parentheses would not be known; they are included here so that we can make a comparison with the mean of all ten items. The number of failures, $k=7$; therefore

$$\hat{\theta} = \frac{1}{7}(2 + 51 + 33 + 27 + 14 + 24 + 4 + 72 + 60 + 21)$$

$$= \frac{1}{7}(308) = 44.0 \text{ days.}$$

The estimated standard deviation is

$$\hat{\theta}/\sqrt{\sum \hat{P}_i} = 44/\sqrt{6.15} = 17.7 \text{ days.}$$

If we had waited until all had failed we should have obtained

$$\hat{\theta} = \frac{1}{10}(388) = 38.8.$$

It is interesting to add that these ten lives were taken at random from a series of 63 for which $\theta = 40.8$. In practice one would aim to have a larger n, and hence larger k, to obtain a more precise estimate.

5. ACKNOWLEDGEMENT

My thanks are due to B. H. P. Rivett who suggested the problem.

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HISTORICAL NOTES ON THE WILCOXON UNPAIRED TWO-SAMPLE TEST

WILLIAM H. KRUSKAL
*University of Chicago**

1. INTRODUCTION

THIS note is supplementary to an earlier review [10, Sec. 5.3] of Wilcoxon's test for the null hypothesis that two unpaired samples arise from the same population. The following topics are discussed: a proposal by G. Deuchler in 1914 for a technique equivalent to Wilcoxon's test, a related paper of the period by O. Lipmann, J. W. Whitfield's 1947 approach to Wilcoxon's test, C. Gini's related concept of transvariation, and G. Ottaviani's work on sampling distributions for transvariation.

2. DEUCHLER (1914)

In 1914, Gustav Deuchler [3, pp. 118–31] suggested an approach essentially the same as that later used by Mann and Whitney in their treatment of the Wilcoxon test. Deuchler's work in this area seems to have been quite neglected, and one purpose of the present note is to redress in some measure this neglect.

Suppose that the two samples are of sizes n and m, respectively. Consider all nm ordered pairs with first member an observation from the first sample (size n) and second member an observation from the second sample (size m). Deuchler suggested scoring, for each pair, $+1$, -1, or 0, as the first member is greater than, less than, or equal to the second member. Then he considered the test statistic

$$\mathfrak{r} = \frac{\text{sum of scores}}{\text{sum of absolute values of scores}}.$$

The denominator is, of course, the number of pairs in which the two members are unequal; if there are no inter-sample ties, this is mn. The relation between $\mathfrak{r}$ and the Mann-Whitney U, in terms of the orientation of [10, p. 603], and supposing no intersample ties, is

$$\mathfrak{r} = 1 - \frac{2}{mn} U.$$

The following two paragraphs [3, p. 119] are a free translation of Deuchler's introduction to his statistic $\mathfrak{r}$:

> "We shall develop the first coefficient in the context of a specific problem of general importance. Suppose that the difference between the achievements of two groups of students, A and B, containing n and m students respectively, cannot readily be

* Part of this research was done at the Department of Statistics, University of California, Berkeley; the remainder was done, under the sponsorship of the Statistics Branch, Office of Naval Research, at the Department of Statistics, University of Chicago. Helpful suggestions have been made by Corrado Gini (Rome), Leonard J. Savage (Chicago), and W. Allen Wallis (Chicago).

quantitatively measured; but that, given any two students, we can order them as to level of achievement. Such orderings correspond the more nearly to objective fact as the achievements are of a simple nature; for complex achievements the question always arises about whether an ordering corresponds to a definite underlying magnitude. But such questions will not be pursued further here. Suppose, then, that the students' achievements are comparable, even if only crudely, so that for each pair of students we can say that one's achievement is better than the other's, or that they are equal. The n students of group A will not be compared with each other, nor will the m students of group B be; thus we may use the symbols A and B to refer generally to the students of the two groups. It might be, for example, that group A consists of the more intelligent students, while group B consists of the less intelligent. Then I ask about the relationship between intelligence and achievement. Practical problems of this type frequently arise in which one can divide the experimental subjects into two distinct groups (A and B) and then compare each student of one group with each student of the other. For example, all achievements that are expressed directly in terms of pairwise comparisons, or all achievements that are graded into two classes, may be so characterized.

"If the relationship between two achievements is such that $A > B$, we write down a plus sign; if it is $A < B$, we write down a minus sign; if it is $A = B$, we write down zero. The zero might be regarded as the average of the plus and minus signs. Thus we have complete correspondence between higher intelligence and higher achievement when only plus signs appear; the correspondence is lower according as the number of minus signs that enter; and if only minus signs appear then intelligence and achievement are in complete opposition. Denote by i the number of plus signs and by k the number of minus signs. We let $t = i + k$ and set

$$r = \frac{i - k}{i + k} = 1 - \frac{2k}{t} = \frac{2i}{t} - 1$$

as the definition of a coefficient with the desired properties. It takes values symmetrically about 0 between $+1$ and -1, and it expresses the level of the mutual relationship in its positive or negative form."

Deuchler then discussed the mean and variance of r, in order to obtain tests and interval estimates. He correctly computed the expected value of r under the null hypothesis that the two samples are from the same population: zero. He found that $1/(mn)$ is the variance of r under the null hypothesis, assuming continuity. The correct value, however, is $(m+n+1)/(3mn)$. Deuchler's error was in treating the scores stemming from the mn pairs as independent Bernoulli variates, whereas they are in fact dependent.

Further topics considered by Deuchler concerning the Wilcoxon test include: the averaging of r's from several separate pairs of samples, the use of a simpler statistic, when $m = n$, based on only m pairwise comparisons (with the method of pairing unspecified), the normal approximation to the null distribution, and the mean and variance under alternatives. The variance under alternatives was computed erroneously just as was that under the null hypothesis.

The scoring scheme used by Deuchler is based on earlier suggestions by G. T. Fechner [4] and G. F. Lipps [13]. It is equivalent to that used later by M. G. Kendall and others in connection with rank correlation coefficients. In fact, Deuchler himself discussed [3, pp. 145 ff.] the rank correlation coefficient now known as Kendall's τ; it had been proposed earlier by Lipps [13], and Deuchler investigated it in more detail. Further discussion and bibliography on this aspect of Deuchler's work will be presented elsewhere [7, 11].

Deuchler refers repeatedly in [3] to a monograph he had written on rank and order correlation as a source of greater detail and fuller explanation about his proposals. Through the courtesy of Professor Richard Kienzle (Tübingen—where Deuchler had been teaching at the time [3] was published) I learned that the monograph never had been published and that the manuscript was in the hands of Professor Deuchler's widow, Mrs. Olga Deuchler (Hamburg). Mrs. Deuchler kindly loaned me the manuscript; it contains a section on the Wilcoxon test that does elaborate on several points made more briefly in [3]. A distinction of possible interest is made at the end of this section between what are called by Deuchler "regular dependence" and "irregular dependence." While the exact role of this distinction is not completely clear to me, "regular dependence" seems to refer to the usual assumption that the two samples are random samples (observations independent and identically distributed within samples) from two distributions. "Irregular dependence," on the other hand, seems to mean that the samples may be made up of strata from different distributions with, for example, half of the A sample coming from one distribution and half from some other. This, however, is an interpretation of Deuchler's intent; his actual discussion says that, if $X_1, \cdots, X_n$ and $Y_1, \cdots, Y_m$ are the two samples, then "regular dependence" means that Pr $\{X_i < Y_j\}$ is independent of i and j, while "irregular dependence" means that Pr $\{X_i < Y_j\}$ depends on the subscripts.

The following paragraphs summarize Deuchler's career:

Gustav Adolf Deuchler was born in 1883 in the Baden region of Germany. His higher education was at the Universities of Heidelberg, Jena, and Leipzig, and in 1909 he obtained his Dr. Phil. degree under Wilhelm Wundt at Leipzig. From 1910 to 1923 he held faculty positions in the fields of psychology and pedagogy at the University of Tübingen. In 1923 he became Professor of Education at the University of Hamburg, where he remained during the rest of his career. He died in 1955.

Deuchler's major interests were experimental pedagogy, the use of ranking methods in statistical analysis, and the training of teachers. He was influential in the movement towards education at the university level for elementary school teachers in Germany.[1]

3. LIPMANN (1908)

Another article of the period, on two sample tests, by Otto Lipmann [12] is also of interest. Lipmann proposes, for the case $m = n$, a procedure virtually the same as one proposed earlier by Galton (see [8] for discussion). The method is then extended to unequal sample sizes. Lipmann does not distinguish clearly between the paired and unpaired two-sample situations. He gives no distribution theory.

4. WHITFIELD (1947)

The list of independent discoveries of Wilcoxon's test [10] should have included an interesting article by J. W. Whitfield [16]. Whitfield treats the two-

[1] This information has been obtained from correspondence with Mrs. Deuchler and from the following two reference works: *Lexicon der Pädagogik*, Herder, Freiberg, 1952. Pp. 716–7; *The Psychological Register*, Vol. III (ed. Carl Murchison), Clark University Press, Worcester 1932. Pp. 784–5. These contain additional biographical detail together with lists of Deuchler's books and scholarly articles. Further information and bibliography has been kindly given to me by Mrs. Deuchler, and I shall try to make it available to those interested upon request.

sample situation as a "sample" from a bivariate distribution with one variate heavily tied, in fact taking only two values. Which of the two values is taken determines which of the two samples the other variate falls in. He then computes Kendall's S-score (with zero for ties) to obtain $2U-mn$ as test statistic. Whitfield also considers the case where there are ties in both variates, corresponding to inter-sample ties from the usual two-sample viewpoint.

Articles by Kendall [9] and Sillito [15] compute relevant variances under the null hypothesis. Much of the discussion in the three articles cited in this section was independently given later by Hemelrijk [10, footnote 7 and p. 603].

5. GINI (1916) AND OTTAVIANI (1939)

Another circle of ideas and techniques, first put forward by Corrado Gini, should be mentioned here because of its relevance to the Wilcoxon test. In 1916, Gini [5] proposed a concept he named *transvariation*. This concept has several aspects in Gini's writings, but the one most germane here is the following: consider two univariate distributions, and hypothetical independent observations X, from the first distribution, and Y, from the second. Suppose that X and Y have unique medians, Med X and Med Y. Now consider the probability that the order of X, Y is different from that of Med X, Med Y, i.e.

$$\Pr\{\operatorname{sgn}(X-Y) \neq \operatorname{sgn}(\operatorname{Med} X - \operatorname{Med} Y)\} = \Pr\{(X-Y)(\operatorname{Med} X - \operatorname{Med} Y) < 0\}.$$

This is the essential notion of the probability of transvariation. However, Gini's definition of the *probability of transvariation between the two distributions with respect to their medians* appears to be the following variation of the above:

$$2[\Pr\{(X-Y)(\operatorname{Med} X - \operatorname{Med} Y) < 0\} + \tfrac{1}{2}\Pr\{(X-Y)(\operatorname{Med} X - \operatorname{Med} Y) = 0\}].$$

The second probability is added to handle symmetrically those cases in which Med $X=$ Med Y or in which $X=Y$ with positive probability. Note that when Med $X=$ Med Y the above quantity is one; however it may in general be larger than one. (The factor 2 permits the "probability of transvariation" to be greater than one, and hence not necessarily a probability. Perhaps it was inserted to make the probability of transvariation range from 0 to 1 when the distributions differ only by translation.)

The relevance of the transvariation idea to the Wilcoxon test is the following: suppose for simplicity that it is known that Med $X>$ Med Y and that X and Y have continuous distributions. Then the probability of transvariation is just twice $\Pr\{X<Y\}$. But this probability is the quantity estimated by $(mn)^{-1}$ times the Mann-Whitney form of test statistic for the Wilcoxon test. It is of some importance in its own right and has recently been discussed from an estimation viewpoint by Z. W. Birnbaum [1].

The Italian statistical literature contains a number of articles on the transvariation idea. A recent paper in English by Gini [6] gives an extensive bibliography. Papers by Castellano [2] and Ottaviani [12] deal with the distribution of the *sample* analog, and natural estimator, of the probability of transvariation.

The Ottaviani paper [12] is particularly interesting in the light of subsequent work. Ottaviani suggests as an estimator of Pr $\{X<Y\}$ the same quantity later discussed by Mann and Whitney as a test statistic, except that Ottaviani naturally divides the Mann-Whitney U by mn. He obtains the mean and variance of his estimator in general, both for sampling with and without replacement as regards the observations on X and Y. The use of his estimator as a two sample test statistic is not explicitly suggested by Ottaviani.

6. CONCLUDING REMARK

Seven independent proposals of Wilcoxon's test, or of closely related procedures, are now known to me: Deuchler, 1914; Ottaviani, 1939; Wilcoxon, 1945; Festinger, 1946; Whitfield, 1947; Haldane and Smith, 1948; and van der Reyden, 1952. It would not be surprising if still other independent proposals exist, since the test statistic is a very natural one for anyone thinking in terms of order relationships.

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SUMMARIES OF PAPERS DELIVERED AT THE 116TH ANNUAL MEETING OF THE AMERICAN STATISTICAL ASSOCIATION IN DETROIT, SEPTEMBER 7–10, 1956

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The present section contains all available abstracts presented at the 1956 national meeting of the American Statistical Association in Detroit. The sequence of presentation is alphabetical according to author.

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SUMMARIES OF PAPERS

A Statistical Study of Longevity. CHESTER ALEXANDER.

The problem has been raised many times whether longevity can be demonstrated to be a heritable trait, to which a number of people have expressed their opinions that it is decidedly a matter of biological transmission, that is, long-lived parents will produce long-lived children. The author of this research has been engaged for several years in gathering data on family lineages in order to investigate the problem statistically. So far life data have been gathered for more than six thousand cases, composed of parents, their offspring, and a control group of unrelated persons. The following correlations have emerged to date. . . . The project is continuing.

1. 500 parents compared with their 679 offspring	r.	−.01
2. Control group of 600 parents and 921 children (not their own)	r.	.05
3. 272 fathers compared with their 679 sons	r.	.17
4. 205 mothers compared with their 474 daughters	r.	.018
5. 206 fathers compared with their 343 daughters	r.	.05
6. 292 mothers compared with their 621 sons	r.	.11
7. 281 sisters compared with their 309 brothers	r.	.30

Other deductions will also be presented. The total data so far fail to support the original hypothesis, therefore it seems that cultural factors may be important matters in the extension of human life. This problem is being developed in both directions, that is further investigation of the family lineage relationship, and the search for cultural factors which may be influential.

Sampling Theory and Sampling Experience in Latent Structure Analysis. T. W. ANDERSON and R. O. CARLETON.

Given data from a latent class model, the latent probabilities for each item can be estimated as the roots of determinantal equations. In large samples the distribution of the estimates from any such equation is approximately a normal distribution. However, the asymptotic variances are complicated functions of the parameters, and the sample size must be very large for one to hope the approximation to be accurate. To obtain further information about the sampling errors of this estimation procedure an analysis was made of a random sampling experiment consisting of four samples of size 1000 from a model composed of three classes and eight items (computed by the RAND Corporation). The estimates of a given parameter varied considerably; in fact, many could not be probabilities. Accuracy tended to be positively related to the size of the unstratified determinant (leading coefficient of the equation). Averaging estimates corresponding to high sample unstratified determinants gave satisfactory estimates. For this size sample the normal distribution did not seem to be a good approximation to the distribution of estimates.

Our Changing Financial Markets. SHERWIN BADGER.

The present period of stringency in our financial markets is different from those in past periods. Changes have taken place in underlying conditions as well as in the market's structure. Changes in interest rates no longer have as important effects upon the supply of and demand for capital and credit because high income taxes dull the impact of the price of money. One of the most important structural changes is the ever-growing trend toward savings and investing through financial intermediaries such as life insurance companies, pension funds, investment trusts, etc. Nearly 80% of personal savings going into intangibles are now being administered by financial institutions, or nearly double the average in the first three decades of this century. Long term credit which used to be supplied by what was generally called "the bond market" is now supplied through several types of bond markets and through other mechanisms, such as "direct placements," dominated by financial institutions. For example, of the corporate bonds outstanding in the country the percentage owned by life insurance companies jumped from 25% to 58% between 1939 and 1952 while the holdings by non-insured pension funds jumped from 2% to nearly 9%. One consequence has been a narrowing in activity in bond trading. Financial institutions are also becoming increasingly important holders of common stocks. This raises interesting questions involving longer-term matters of public and economic policy.

A Stochastic Approach to Enzyme Kinetics. ANTHONY F. BARTHOLOMAY.

The limits which the classical deterministic mathematical theory imposes on the analysis of mechanisms which have been postulated to account for certain types of enzymatic reactions with obvious random components, such as "inhibition" and "step-reactions," possibly can be overcome by the application of the methods of stochastic processes. A further reason for proposing a stochastic approach to Enzyme Kinetics is more general and is based on the inadequacy of the current kinetic theory to account for the apparent "irreproducibility" of certain chemical reactions. The criterion for the arbitrary classification of reactions into the "reproducible" or "irreproducible" category is empirical and is a function of the sensitivity of the techniques for obtaining kinetic data. However, certain basic mathematical considerations about even the simplest accepted "reproducible" reactions point to the existence of a quality of randomness which is not observable by current experimental techniques. The advantages of a general mathematical approach to chemical kinetics which is an invariant of experimental procedure is obvious.

For these reasons, a general investigation of chemical kinetics from the point of view of the theory of stochastic processes has been undertaken. The "unimolecular," first-order, reaction is the logical beginning point of this study. A "linear-death" type of stochastic process, with finitely many states has been derived for this case. This model satisfies an arbitrary criterion of "consistency-in-the-mean" with respect to the classical theory. Other basic chemical reactions are currently being examined from this same point of view.

Tract Programming for the 1960 Census. CLARENCE E. BATSCHELET.

As the Census of 1960 approaches, the Bureau of the Census is endeavoring to arouse an interest in census tracts in the untracted cities of 50,000 or more population. It is our goal to extend the census tract program to all cities in this size class, but there are still 59 cities in which no census tract work is underway, so far as we are aware. We feel that a word from the users of tract statistics may be all that is needed to arouse a local interest in tracting.

Sample Design with the Sampling Error as a Cost Factor. ERNEST P. BILLETER.

Designing sample surveys always means the balancing out of the sampling error against the sampling costs. Thus the main target in designing a sample survey consists of reaching the lowest possible sampling error for a given cost. The smaller the sampling error the higher the costs of the survey, for reducing sampling error means first of all increasing sample size (for one specific sample design), and, in turn, increasing the costs of the survey. The main task in designing a sample survey consists in weighing the accuracy of the sample, given by the low value of the sampling error, against the costs it involves.

It would be of great advantage to introduce the sampling error as a cost factor, besides the costs of planning and carrying out the survey. For this purpose we state that the costs incurred in taking a sample survey are made up by the costs of planning and carrying out the survey C_1, and the costs incurred by a large sampling error C_2. Thus the overall costs are: $C = C_1 + C_2$. Concerning the component costs we make the following hypotheses: $C_1 = a + cn$, $C_2 = b(\sigma_x^2)^\beta$, where a are the costs incurred in planning the sample survey, c the costs incurred (per unit taken into the sample) in carrying out the survey, b and β are parameters determining the costs which are dependent on the sampling error σ_x^2, and finally n stands for the sample size.

Taking these hypotheses for granted, we compute the overall costs for a simple random sample design as follows: $C = a + cn + b\sigma_x^{2\beta}$. Introducing the sampling error

$$\sigma_x^2 = N^2 \frac{N-n}{N-1} \frac{\sigma^2}{n} \doteq N\sigma^2 \left(\frac{N}{n} - 1 \right)$$

we get as the overall costs: $C = a + cn + bN^\beta \sigma^{2\beta} (N/n - 1)^\beta$.

It is easy to recognise that by increasing the sample size n step by step, the overall costs C first decrease until they reach a minimum value and then increase. The optimal sample design is characterised by that sample size n_{opt} which is given by the minimum of the overall costs. This optimal sample size is easily determined by taking the first derivative of C in respect to n and equalling it to zero. This gives the following expression: $b\beta N^{\beta+1} \sigma^{2\beta} (N - n_{opt})^{\beta-1} = c n_{opt}^{\beta+1}$. Given a specified value for β, n_{opt} can be determined.

Philosophy of an Integrated Federal Statistical Program. RAYMOND T. BOWMAN.

The success with which we as a free society can maintain a high level of economic and social well-being without excessive oscillations or unnecessary inequalities requires quantitative information on the way the economy and its social institutions function. It is obvious that the Federal Government is not the only provider of this quantitative information: business and labor organizations, private and scholarly societies, as well as Federal, State and local governments all have appropriate parts to play. The point of view which should govern Federal statistical responsibility is that of achieving an adequate description and analysis of the functioning of the social and economic life of the nation as a whole.

Our Federal statistical system is an evolutionary product, with many irrational arrangements. It is a decentralized system, with a central coordinating agency (the Office of Statistical Standards in the Bureau of the Budget) to provide the necessary cohesive force and leadership. Six major demands bear with special weight on the Federal statistical program: demands for (1) unlimited detail, (2) comparability, (3) frequency, (4) promptness, (5) high accuracy, and (6) low cost and low reporting burden. It is obvious that some of these demands are contradictory and that they cannot all be met simultaneously. Compromises must be made in the light of the purposes to be served.

The present Federal statistical program must be materially improved in many respects, but the improvements must come by an evolutionary process. This evolution may require some rather revolutionary changes and some reorganization of functional responsibilities. Success in reaching the common goal of an improved Federal statistical program will require active interest and participation by the agencies producing and the agencies using the statistics, as well as wise leadership on the part of the central coordinating agency.

Some Applications of Statistics to Meteorology. GLENN W. BRIER.

Several examples are given illustrating the importance of choosing the proper statistical parameter for use in an analysis of meteorological data. The first example shows that the extreme highest (or lowest) value of a turbidity measurement made during a month is more sensitive than the monthly average value to certain types of atmospheric or instrumental changes taking place over a period of time. A second example shows some advantages in making predictions from time series in terms of the probability that a specific value of a variable x (such as Washington minimum temperature) will be equalled or exceeded within t or more days. It is shown that such probability forecasts can be useful even though estimates of the probability of an event are in error by 10 or 20 per cent.

An Approach to More Sensitive Vaccine Screening Procedures. BYRON W. BROWN, JR. and EUGENE A. JOHNSON.

The safety of a vaccine lot is defined in terms of the proportion of undesirable reactions resulting when the whole lot is administered to test animals in standard doses. Several test procedures for screening vaccine lots for safety using increased doses are described. The test procedures are described in terms of the protection they afford against accepting unsafe lots and against rejecting safe lots. The test procedures obtained are an attempt to obtain practical screening methods which furnish desired levels of protection at a minimum in sample size.

The test procedures described utilize varying amounts of information concerning the dose response function relating proportion of undesirable responses to dose level of vaccine. An optimum sample size plan is described for the case of known logistic response function and known parameter of variation. An adjusted logit transformation is used to obtain an approximately linear regression solution, asymptotically normal, for the case of known logistic response function but unknown parameter of variation. Examples are presented for both situations and comparison of sample size with the screening procedure using standard doses is made. The situation in which the functional form of the dose response curve is known only in its central range and the parameter of variation is unknown is discussed.

Educational Statistics. S. M. BROWNELL.

The Office of Education is one of the oldest statistical agencies in the Federal Government. Its statistics have, in the main, provided overall figures on the educational program of State and local school systems and of institutions of higher education. One of the problems that has confronted the Office is the inconsistency in data reported by various educational sources. Significant progress has been made toward a common understanding of basic items of educational information. A project begun by the Office in cooperation with State Departments of Education, and other groups will result ultimately in the publication of four handbooks. The first presents an agreed-upon common core of items of State educational information for which records should be kept and reported. The others will deal with agreements in terminology and plans for financial accounting, personnel accounting, and property accounting.

A newer phase of the Office program is the Cooperative Research Program, supported by an initial Congressional appropriation of approximately one million dollars. This program stems in part from the belief we are not doing nearly enough research on problems in the field of education to insure that we will solve our immediately pressing problems, let alone be prepared for future developments. The staff of the Office of Education has recognized the need to move ahead in three major areas of educational research. First, to have undertaken researches into problems inherent in the housing and staffing of the Nation's schools and colleges.

Secondly, to have research carried on which will make possible more efficient conservation and development of human resources through education. Thirdly, to examine the implications of our expanding economy and advancing technology for the future of education. Research planning is designed in such a way as to enlist the talent and interest of colleges, universities, and State departments of education in the hope that it may capitalize upon research talent where it exists in the greatest quantities, and stimulate additional research efforts all over the country.

Population Census Plans. HOWARD G. BRUNSMAN.

Serious consideration is being given to the following changes in treatment of areas in the 1960 censuses of population and housing: 1. Establish urbanized areas for each incorporated place of 25,000 or more (such areas were established for places of 50,000 or more in 1950). 2. Publish statistics for separate unincorporated places within the fringe of urbanized areas. 3. Publish at least total population for unincorporated places of 500 or more. 4. Promote the establishment of census tracts for all areas within all of the standard metropolitan areas and publish statistics for all such census tracts. 5. Establish census county divisions in about 14 of the states where existing minor civil divisions are either not permanent or are not locally recognized.

The following questions regarding the content of the census of population are receiving serious consideration: 1. Should information on mobility relate to one year (as in 1950) to five years (as in 1940), or to 10 years? Should such statistics attempt to measure movement between central cities of standard metropolitan areas and the remainder of such areas? In the tabulations should more emphasis be placed on migration within standard metropolitan areas? Should less attention be paid to State economic areas in the migration tabulations? 2. Should information be obtained and tabulated on place of work in relation to place of residence? It has been proposed that these data be obtained at least for the central cities of standard metropolitan areas and by counties for the remainder of such areas. 3. Should college students be counted as part of the population of the place where they are attending school or of the place of residence of their parents? 4. Should economic activity be based on a longer time-period, such as a month or year, instead of the one-week base of 1950? 5. Should we continue to cut back on the attention given to the foreign stock as a separate group; and, if so, in what ways?

Use of SEAC for Statistical Calculation. J. M. CAMERON.

Examples of the use of SEAC for statistical calculation are presented. In calculations using eight significant digits the effect of round-off on the accuracy of the final answers is discussed and different methods of calculation are compared with respect to the accuracy attainable for certain standard statistical problems.

The Requirements of Scientific Computations Arising in Government Computing. J. M. CAMERON and M. ABRAMOWITZ.

This talk gives a survey of the nature of computational tasks arising in a scientific computing center. Some examples of statistical and other computations are presented. Qualifications desired of numerical analysts and programmers are discussed.

Screening in the Food Industry. M. B. CARROLL.

One of the major problems in the food industry is the screening of judges. A procedure is outlined using triangle tests for screening that (1) does not require that specified judges test together, (2) adjusts for the degree of difference in the samples tested by a judge, and (3) allows for establishing a panel of any required size. The procedure is such that research information is provided on the samples at the same time as the judges are being screened.

A set of normalized scores are obtained for the judges and on the basis of their distribution a measure of the ability of each judge is obtainable. A graphical method of studying the distribution of normalized scores is presented. The procedure is illustrated with the results of 57 taste tests involving 104 judges.

On the Solution of the Functional Equation of Farrell's Market. A. CHARNES and O. P. AGGARWAL.

A. Charnes and M. Farrell in June 1953 derived à la R. Bellman a functional equation describing the optimal pricing strategy of a seller in a recurring market of Markovian reaction characteristics of "kinky oligopoly" type suggested by Farrell (*Econometrica* v. 22, No. 3, July 1954) in his approximate "im kleinen" analysis of an econometric controversy on optimality of holding price constant. They established (unpublished) existence and uniqueness of continuous solutions of at most exponential growth for this functional equation, a type still not subsumed in any yet treated (cf. S. Karlin, *Naval Research Logistics Quarterly* v. 2, No. 4, Dec. 1955). This paper develops explicitly the exact solution for all relevant parameter ranges. Hence (1) a new method of approximately (or exactly) solving "extremal" functional equations by employing piece-wise quadratic "forcing" terms is suggested and partially implemented, (2) an "im grossen" resolution of the econometric controversy is at hand.

Problems in Developing an Integrated Federal Statistical Program. EWAN CLAGUE.

In 1933–34 the Committee on Government Statistics and Information Services made a comprehensive study of Federal statistics. Some recommendations of that Committee are still good today. One was that the division of labor and decentralization of authority among the various statistical agencies should be maintained, if the statistical services are to be responsive to the practical needs of administration and policy making. Another was a recommendation for a central statistical board to effect the necessary coordination. The Office of Statistical Standards is the present-day counterpart. Effective coordination has been achieved over the past 20 years in some programs—for example, employment statistics. The Census, Labor Statistics and Employment Security bureaus, together with 48 State agencies, are all involved. Nevertheless, a coordinated system has been worked out under the auspices of the Office of Statistical Standards so that a combined release is now issued monthly. Despite many successes, a number of serious and difficult problems remain. One is the task of bringing the large administrative agencies into better coordination with the statistical agencies. Another is establishing and maintaining the highest standards of integrity in Federal statistics. The growth of State statistics intensifies the problems of coordination because it results in Federal-State systems. The widespread use of statistical indexes in escalation requires the maintenance of continuity and accuracy of the series. In the immediate future, the introduction of the new Standard Industrial Classification will involve many agencies and require the greatest possible coordination.

The Engineering Applications of Statistics. DAVID N. COLLINS.

Mathematical statistics is useful to engineers when the artificial manipulations of statistics can be made to correspond with variations among important physical properties of the sytem under examination. The results of the analysis can then be expressed numerically as probabilities of real differences among the variables analyzed. By application of statistics, results of ordinary precision can be obtained with less laboratory work, or precision can be increased at little or no cost in extra laboratory time.

The three operations of statistics most commonly used in the laboratory are the analysis of variance, calculation of the probability of a difference between two averages, and correlation measurements. Using the analysis of variance, the effects of measured variables are separated from uncontrolled experimental error; the resulting parts can then be evaluated separately. The difference between two averages is expressed as a function (Student's t) of the combined variance of the original data. Correlation makes quantitative the straight-line relationship (if any) between two variables, or measures the contribution of each of several independent variables on the end result.

Although statistics, as applied above, makes possible the condensation of a series of measurements into a single quantitative result, its applications are most effective before laboratory work begins. In the design of experiments, the statistician acts as a referee, advising and arbitrating between the man who will do the laboratory work and the man who asked the question which the work must answer.

Before a statistical design can be selected, it must be agreed that the question asked has been expressed in terms that the laboratory worker can understand. In addition, the statistician must be furnished with a numerical value for the reliability of the test method, and a value for the least difference in results which will be of practical value to the user. The outcome of a design conference should be the selection of a type of test program and the preparation of a detailed schedule of testing.

Notes on the Distribution and Analysis of Income by Source. ROYAL A. CRYSTAL.

This paper presents a method of estimating and distributing a firm's income by source. Several uses of such an analysis are suggested as applicable to situations which may be found in many business organizations. The method presented has been used with much success in the insurance field and with some modification is adpatable to various operations requiring a rapid and accurate analysis of income.

Emphasis is placed on the methodology and sequence of computation, expanding the equation $(E)(p) = Ep$ to accommodate any number of items while remaining able to check any element or series of elements for accuracy during any stage of computation. In stressing the effectiveness of the method, attention is noted to the fact that over a thirty-month period accuracy of computation was maintained at better than 99.50% with a standard deviation of .071%.

The paper stresses the working union of the statistician and accountant in performing the various audits of accounting data which are required in verifying the inference drawn from the distribution. An underlying thought is that the method of computation provides management with a tool which can be used as a basis for highly diversified cost analysis in many areas of activity.

American Farmers and the St. Lawrence Seaway. WILLIAM DACHTLER.

American farmers, especially those in the North Central States and in Pennsylvania and New York, have a considerable interest in changes in agricultural marketing that will be brought about by the St. Lawrence Seaway.

Construction of navigational improvements is under way that will permit large oceangoing vessels to enter the Great Lakes in 1959. Products of agriculture produced in the Midwest and transported to the eastern seaboard will then have an alternative route possessing distinct possibilities of lower overall transportation costs than the rail or combination rail-lake route now used.

A number of questions must be answered before an assessment can be made of how extensive these savings may be. The boundaries of the territory that will be tributary to the Seaway are indefinite. The number of agricultural commodities which the Seaway may serve is not fully known and the potential volume of traffic for each is unknown. Existing handling, storage, processing, and transportation arrangements are interrelated. The extent of change that is likely to take place within this framework is yet to be ascertained. Another open question is whether the railroads will establish rates that will compete with the Seaway should it appear that the latter route will be cheaper for shippers.

Plans for the 1960 Housing Census. WAYNE F. DAUGHERTY.

Overall planning for the Census in terms of what things should be done and how to execute the principal steps in the program are rather definite. We are intently concerned with learning from the users of census statistics what will be most useful to them in future years. Priorities must be established based on need, costs of collection, etc.

There are a number of fundamental issues to be settled—purposes and aims of the 1960 Census, questions of coverage, scope, concepts, and comparability. Should the Housing Census be geared to serve the greatest number of users, specialised users, or the most important users? Should it recognize that the needs of government users may differ from those of private users? The design of a census depends on the extent to which the purposes and aims can be recognized and resolved. The question of coverage in the census is a fundamental one. In the 1950 Census, living quarters in transient hotels, tourist accommodations, rooming houses, etc., were excluded from the housing inventory. Should the 1960 Census be expected to include such quarters?

Another issue which is very basic to the 1960 Census relates to definitions and concepts. The principal ones are: In 1950 the enumerators were instructed to identify each dwelling unit as "dilapidated" or "not dilapidated," for the purpose of identifying that part of the supply not providing minimum shelter. Since this classification appears to be inadequate to describe blighted or deteriorating neighborhoods, should this dichotomy be abandoned in favor of more gradations. Cost of housing is one of several economic measures being considered. This has been available for rental units as either contract or gross rent. For owners, data on taxes and utilities may be more useful than value of homes as collected in 1950. There are problems in a descriptive classification of rural housing, and doubts have been raised as to the meaningfulness and usefulness of the present form of classification. Measurement of space is becoming difficult. With the trend toward multiple-use rooms, the definition of a room becomes ambiguous. Would a measure in terms of the number of bedrooms be an alternative? If one of these measures is not satisfactory, should the subject be dropped? Last, is the concept of measurement and description of housing markets. It has been proposed that selected data be obtained on previous residence for persons who have recently moved—location, whether owners or renters, and the rent or value of their former home.

The topic of comparability of data from census to census is being given a great amount of attention by the Bureau staff. Sometimes even a slight change in the wording of a definition or concept disturbs the user. On the other hand, using the same definitions and concepts does not necessarily assure identical data from time to time. Housing statistics is a relatively young field, and interest in housing by sociologists, economists, and by the government is recent. It has resulted in discarding some of the old concepts, and exploring and adopting some new ones—a process which is likely to continue for many years. In prejudging the situation in 1960, it is highly unlikely that our resources will be adequate to fulfill all the needs of all types of users.

Control of Precision in the Plate Count Assay. IRA A. DEARMON.

In estimating the number of viable organisms in a sample concentration of bacteria, the measurement consists of the plating operation and the serial dilution operation. The variability of each of these operations has been evaluated for a fastidious microorganism and has been shown to be consistent with that variability expected in a Poisson distribution. Utilising the χ^2 distribution, a control procedure has been suggested whereby both the plating operation and the serial dilution operation can be assessed for excessive variability on the same control chart. This control procedure has been applied with success to an actual program carried out in a biological laboratory.

Matching Transactions Improves Predicting. STUART C. DODD.

The Problem: The "transact model" is designed to improve the predicting of mass behavior, with elections as an available test case. This paper is not a review of the topic, but instead offers a systematic operationally defined theory for predicting. Any poller can use it this fall although it is only incipiently tested as yet in election situations.

A Proposed Solution: To maximise all predictive correlations between a predict and transaction (i.e., situation, such as voting in November) and a predicter transaction (such as pre-election polls), *match all their factors, facets, and functional relations.* "Matching" means finding predicter situations which are most similar in all respects to the predict and situation. The relevant respects can be standardised under fourteen headings, viz: 1) The six factors or categories of variables below; 2) The four facets or aspects of each factor; and 3) four functional relations or ways of combining all these.

Every human transaction, defined as action-in-full-context, can have that context factored into its *actors, timing, spacing, values* or desiderata, and residual *circumstances.* Every factor has four facets, namely: It is represented by some observed *index,* at some *power,* in some *degree,* with some critical *points.* All these factors and facets may be functionally combined as *sets, sums, products,* or *powers,* or as their inverses. In these fourteen respects the more the polling when symbolised matches the public's voting behavior when symbolised, the better the prediction will be. This testable expectation is the set of "matched transacts" hypotheses.

Screening Problems in the Pharmaceutical Industry. CHARLES W. DUNNETT.

The pharmaceutical industry as well as various public and private research organizations devote considerable effort to screening. The items screened may be chemical compounds or cultures of microorganisms. In all cases, the object is to select for further testing those items which appear to possess some specific property, or to possess that property to a greater degree than normal. Since there are usually more items to be screened than can be adequately tested, it is necessary to effect a compromise between testing a small number of items adequately or testing a large number inadequately. Specific examples discussed are (1) screening chemicals for possible use in the treatment of cancer, (2) strain improvement research in antibiotic production and (3) searching for new antibiotics.

Basic Instruction in Statistical Computations. P. S. DWYER.

The paper describes basic instruction in statistical computation as it has been and now is at the University of Michigan, discusses certain topics which should be considered in formulating a program in basic instruction in statistical computation, and makes several suggestions for such a program. Specifically it discusses such topics as the varied instructional programs of a large university, training in the various programs at Michigan, computational equipment at Michigan, the nature of statistical computations, the use of digital machines, the control of computational error, the use of direct or iterative methods, the accumulation of inherent error, and terminates with general remarks regarding a suitable program of basic instruction in statistical computations.

A Period of Expanding Use of Tract Statistics 1946–56. A. ROSS ECKLER.

In the past 10 years the number of cities in which census tracts have been established has more than doubled and the rate of expansion has been even more rapid in the suburban areas adjacent to the large cities. The expanding use of census tract statistics in the past 10 years has been due in considerable measure to the more active utilization of such statistics by business organizations and planning agencies. More than half of the key persons, who serve as chairmen of local tract committees and generally stimulate the tract program, are now drawn from planning agencies and from business organizations, including Chambers of Commerce. These types of organizations were responsible also for more than half of the census tract publications issued between 1950 and 1954. In earlier years, by contrast, local municipal departments and social and welfare agencies were the most important users of tract statistics. The annual meetings of the Census Tract Committee of the American Statistical Association have given much emphasis to business applications in the past 10 years. This development was doubtless stimulated by the fact that special tabulation areas, typically consisting of combinations of census tracts, were established in a number of cities in both the 1948 and 1954 Censuses of Business. Large-scale population shifts affecting both the numbers and kinds of people in subdivisions of cities and in local developments outside cities have greatly stimulated the use of census tract statistics and promise to continue to do so. The Census Bureau recognizes the importance of the functions which have been transferred to it from Howard Whipple Green in the past several years, including responsibility for encouraging the extension of tracting, the designation of key persons, and the maintenance of a library of tract publications. It hopes to carry on these functions successfully and will strive to achieve maximum progress toward the 1960 objective of establishing tracts for all cities of 50,000 or more and for all Standard Metropolitan Areas with a central city of 100,000 or more.

Techniques and Uses of Forecasts of General Business Conditions. IRA T. ELLIS.

The function of an industrial economist is to take a broad view of economic trends in the country or in the world. He will acquire experience in fields of particular interest to his company, or in his particular industry, but he should be first of all an economist, not a market analyst or a sales forecaster.

The industrial economist studies history to see what has happened and why, as a clue to what present trends may suggest for the future. He must express his estimates of the future in general terms, particularly as the look into the future lengthens, because history never repeats itself exactly. The economist looks at statistical measures of inventories, prices, business investment, residential construction, employment, personal income, and population. He evaluates international developments, domestic political developments and the state of personal or business confidence. He leans heavily on trends in projecting his thinking into the future beyond the next year or two.

Let us turn now to uses of general business forecasts in the du Pont Company. Sales forecasting in du Pont is decentralized. Each of the ten manufacturing departments is responsible for its production and sales. Sales forecasts, therefore, are prepared in the manufacturing departments.

Forecasts of general business activity for the next two years are prepared in the Economist's Office in July, for use by the departments in preparing their general sales forecasts for the final quarter of the current year and for the next two years. The Economist's general business forecasts cover principally industrial production, wholesale prices, raw materials prices and average hourly earnings in manufacturing.

We prepare in November a detailed forecast of the expenditures side of Gross National Product for the coming year, together with estimated production rates in selected industries. These forecasts are used by the departments in preparing the detailed Company sales forecast for the following year.

There is also a demand for long range forecasts of the business outlook for use in financial planning or for other planning at the general management level. Here the questions are largely in the field of monetary policy, interest rates, construction costs, and prices.

Forecasts should state explicitly the assumptions upon which they are based, particularly those concerning business investment, defense expenditures, price changes, etc. Above all, the forecast should be objective.

Determining the Effect of the St. Lawrence Seaway on Industrial Growth in the Great Lakes Area. B. E. ESTES.

Ordinary statistical methods are of little value in determining the effect of the St. Lawrence Seaway on industrial growth in the Great Lakes Area. A usable answer can come only through the exercise of judgment, aided

by statistics and analysis, but devoid of anything which could be called methodology or the application of a formula. It would be extremely difficult even to measure the industrial growth after the fact, and it would be still more difficult to identify that part of the growth engendered by the Seaway. The analysis might logically deal separately with three specific categories of influences—two favorable, the third possibly unfavorable. These are as follows: 1. The favorable effect of new raw materials, or raw materials from new sources—available in larger quantities, better qualities, and at lower delivered costs. 2. The favorable effect of being able to ship at lower costs to certain markets, both foreign and domestic. 3. The possibly unfavorable effect on the area of new competition from products shipped via the Seaway from foreign countries or from the eastern part of the United States.

One final factor in the analysis is extremely important. While the Seaway will provide considerable impetus to industrial growth in this area under any sort of economic conditions, it seems that the greatest and most rapid growth would be possible during a period when our economy was growing and expanding. Any analysis must, therefore, include a full statement of the sort of economic climate expected.

Even upon completion of the very sizable statistical and analytical task laid out, the job of synthesizing this into a useful answer through qualified judgment yet remains. This answer would be most useful if it did not attempt somehow to conclude that during the next ten years industrial growth in the Great Lakes Area will be accelerated by x per cent as a result of the Seaway. The most useful type of answer would indicate the industries and areas most likely to benefit by the Seaway, as well as any which might be less favorably affected.

Covariance Analyses for Unbalanced Two-Way Classifications. W. T. Federer.

Covariance analyses for two-way classifications with unequal numbers of observations in the subclasses may be classified as follows: Case I—Interaction absent. Case II—Interaction present; the effects assumed to be fixed effects. Case III—Interaction present; the interaction effects and one or both of the main effects assumed to be random effects.

A Case I analysis variance has been designated as "the method of fitting constants" in the literature. A Case II analysis of variance has been called "the weighted squares of means" analysis. A Case III analysis has not appeared in the literature except in abstract form.

The linear models for the three cases are: *Case I:*

$$Y_{ijk} = \mu + \tau_i + \rho_j + \delta_{ijk} + \beta(X_{ijk} - \bar{x})$$

Case II:

$$Y_{ijk} = \mu + \tau_i + \rho_j + \tau\rho_{ij} + \delta_{ijk} + \beta(X_{ijk} - \bar{x})$$

Case III:

$$Y_{ijk} = \mu + \tau_i + \rho_j + \tau\rho_{ij} + \delta_{ijk} + \beta_1(\bar{x}_{ij.} - \bar{x}) + \beta(X_{ijk} - \bar{x}_{ij.})$$

where μ = an effect common to all observations, τ_i = an effect common to the ith level of the first factor or classification, ρ_j = an effect common to the jth level of the second factor, δ_{ijk} = a random error associated with the ijkth observation, β and β_1 are regression coefficients of Y on X from the within subclasses and error lines in the analysis of variance, and the interaction effects are defined above.

For a Case I analysis the number of observations per subclass, n_{ij}, may equal 0, 1, 2, $\cdots$. However, for a Case II analysis, n_{ij} must be greater than or equal to unity if the usual restrictions are used. If zeros are inserted for the interaction effects where $n_{ij} = 0$, an approximate analysis is possible. The closeness of this approximate analysis to the correct analysis depends upon the number of $n_{ij} = 0$ and upon the size of the true interaction effects.

For a Case III analysis, the value of the weights w_{ij} will depend upon the assumptions made about the data. The true variance of a subclass mean y_{ij} is $\sigma_\epsilon^2 + \sigma_\beta^2/n_{ij}$. If the subclass means are weighted inversely to their true variances then $w_{ij} = n_{ij}/(\sigma_\beta^2 + n_{ij}\sigma_\epsilon^2)$. Now if $n_{ij}\sigma_\epsilon^2$ is small relative to σ_β^2 then w_{ij} is essentially n_{ij}. If, on the other hand, $n_{ij}\sigma_\epsilon^2$ is large compared to σ_β^2 for all n_{ij}, then w_{ij} is essentially equal to unity. However, the true situation is usually in between these two limiting situations, and no solution is possible unless σ_ϵ^2 and σ_β^2 (or $\sigma_\beta^2/\sigma_\epsilon^2$) are known. An approximate solution is possible if the estimates $\hat{\sigma}_\epsilon^2$ and $\hat{\sigma}_\beta^2$ from the experiment (or from another one) are used for the population variances σ_ϵ^2 and σ_β^2. If the variance components are estimated from the data, it is suggested that the analysis of variance be performed interatively, obtaining a new estimate of σ_ϵ^2 from each analysis. The process should be continued until the w_{ij} tend to stabilize. This approximation may not be too bad if sufficient degrees of freedom (say greater than 20 to 30) are available for the within subclasses and the error sums of squares. Also, if any $n_{ij} = 0$, the corresponding $w_{ij} = 0$.

Electronic Potentials for Generating, Analyzing, and Presenting Economic Data. Frank R. Garfield and Maurice H. Schwartz.

Further improvement in economic analyses to be presented to policy makers in positions of private or public trust may be sought from many sources, including electronic computing. In this paper, attention is focused on the direct effects of electronic computing on the generation, analysis, and presentation of economic data, with only passing reference to possible indirect effects stemming from changes in inventory and other business policies which may result from electronic computing.

Gaps in data and deficiencies in timeliness, quality, and detail of existing data for analysing current economic developments are reviewed, and brief mention is made of continuing problems of analysis and presentation. Then, note is taken of selected facts concerning automatic electronic computing equipment and the use thereof by various types of private and public agencies for many different purposes. Brief reference is made to an electronic charting machine being developed for the Federal Reserve Board. Suggestions are offered concerning ways in which electronic computing may facilitate improvement in economic analyses, with emphasis on improvement in basic data, often possible at low cost on a by-product basis. Finally, consideration is directed to the various roles that statisticians dealing with business and economic problems may play in guiding the activities of these machines—and vice versa.

A growing number of business statisticians acquiring some understanding of computer potentials will be in a better position to help shape data programs to provide more adequate internal statistics to management and to improve the flow of information to national and regional data-compiling agencies. With increasing aid from electronic computing, the data-compiling agencies in turn may well be able to improve further their planning of reports and their processing of data received. Scholars in academic communities and research organizations may make major contributions at many points, as heretofore. Business and economic statisticians as Association members may well consider what part the Business and Economic Statistics Section and the Association as a whole should play in making electronic computing as effective as possible for the accomplishment of broad statistical purposes.

In conclusion, the authors urge that any efforts to advance understanding in the economic field, and other fields as well, through electronic computing should be undertaken with full recognition that the maximum potential gains —in statistical methodology as well as in subject-matter interpretation—are to be achieved only through intensive study and experimentation by both those primarily interested in subject-matter problems and those primarily interested in problems of statistical methodology.

A Psychometric Analysis of Employee Performance. JAMES P. GEORGE.

Employee efficiency ratings afford a basis for the formulation of administrative policy and for personnel action. Employee performance records are consulted whenever promotions, reductions in rank, transfers, or separations from the service are being contemplated. The establishment of a norm or standard of accomplishment is, therefore, desirable. The present paper reports findings in consequence of an analytical study of Navy Department numerical efficiency ratings for the year, ending March 31, 1943.

Mathematical functions or frequency curves were fitted to an overall distribution of numerical ratings of 15,178 Civil Service employees of the Navy Department. In one instance a descending exponential in combination with a simple parabola was fitted to the distribution of ratings. In another instance the Pearson Type III frequency curve was so fitted.

Analysis of the observed distribution of Navy Department numerical efficiency ratings shows it to conform to the Pearson Type I criterion. The calculated value of Chi square turns out to be 0.4454. By many this fit would be looked upon as being almost too good.

Some allowance must be made in the interpretation of the Chi square test when applied under such unusual circumstances, however. A value larger than 0.4454 can be expected to occur in 95 cases out of 100. The assumption that this mathematical function presents a faithful portrayal of the universe of Navy Department efficiency ratings can in the judgment of the writer be considered tenable.

How well does the Pearson Type III frequency function fit the sample of numerical efficiency ratings? The 95 per cent tabular value of Chi square is 1.145. The calculated value of Chi square is 0.8310. This is a very good fit. The conclusion appears justified that, as an acceptable description of the universe of Navy Department numerical efficiency ratings, there is no reason for the rejection of the Pearson Type III frequency function.

In general any frequency distribution can be quite satisfactorily described in terms of five statistical measures. Analysis of the observed distribution of Navy Department numerical efficiency ratings, and of each of the two hypothetical distributions, yields the following descriptive measures, $\bar{x}=2.8$; $\sigma=1.5$; $\alpha_3=.9$, $\alpha_4=3.9$.

Job Evaluation through the Medium of Multiple and Partial Correlation and Regression Analysis. JAMES P. GEORGE.

The present study represents an attempt to explain salary level in terms of the sum of weighted scores. The scores referred to are those assigned to various elements, pertaining to a particular position. Known as standard allocation factors, these eight elements are descriptive of different aspects of the job. The major objective is an appraisal of the standard allocation factors as to their combined action, and as to their relative importance in accounting for variation in salaries paid Civil Service employees of the Navy Department. A secondary objective is the derivation of a regression equation which will serve as an instrument of prediction of the salary to be paid as determined by a given set of scores.

The information upon which this investigation is based was made available to the Statistics Section, Shore Establishments and Civilian Personnel Division, through the courtesy of the Chief of the Classification Branch. In their original form these data showed evaluation pattern, grade, service, and salary of 230 Civil Service employees of the Navy Department. The scoring of these jobs was done by seventeen carefully selected classifying officers, who had received intensive instruction and training in preparation for this assignment. For the purposes of this study 141 positions were drawn from the larger number indicated above. These positions represent a 1.5 per cent stratified sample of the universe.

The investigation consists essentially of a thoroughgoing statistical analysis of the relationships and interrelationships between dependent variable (X_0) SALARY in hundreds of dollars, expressed in terms of logarithms, and a series of eight independent variables. The associated independent variables or standard allocation factors, include: (X_1) Supervisory control exercised over the position, (X_2) Availability and nature of guide lines, (X_3) Originality of thinking required, (X_4) Variety, (X_5) Purposes and nature of personal relationships, (X_6) Nature and scope of commitments, (X_7) Nature and control of work of others, and (X_8) Number and level of positions controlled.

The findings resulting from this investigation were successfully applied by the Navy Department in the determination of salary standards in position classification, and also as a basis for formulating policy in the allocation of positions by grade and class. On the basis of eight independent variables, 83.9 per cent of the variation from the line of average relationship has been accounted for as it relates to salaries in 141 positions.

On the average, salary tends to increase by $185.29 for each unit of increase in variable (X_1) Supervisory control exercised over the position; by $216.30 for each unit of increase in variable (X_2) Availability and nature of guide lines; by $165.12 for each unit of increase in variable (X_4) Variety; and so on for the remaining associated independent variables.

In contrast with the general tendency for the relationships to be direct, it is well to observe what happens in the case of variable (X_3) Originality of thinking required, and in the case of variable (X_5) Purposes and nature of

personal relationships. In the former instance a decline, amounting to $136.33 in salary tends to be associated with an increase of one unit in that particular independent variable. In the latter case a decline of $144.30 tends to accompany an increase of one unit in this independent variable.

In order to answer the question of statistical reliability, Fisher's "t" test is applied to the Beta coefficients, and to the partial correlation coefficients of the seventh order. Under the conditions of this problem, with 132 degrees of freedom, comparison of calculated "t" values with tabular "t" values, leads to the following conclusions: Variables (X_3) Supervisory control exercised over the position, (X_4) Availability and nature of guide lines, (X_5) Nature and scope of commitments, and (X_6) Number and level of positions controlled, are of high statistical significance, i.e., 99 per cent probability. Variables (X_1) Variety, and (X_2) Purposes and nature of personal relationships, are of moderate significance, i.e., 95 per cent probability. From the evidence at hand it appears as though (X_8) Originality of thinking required, and (X_7) Nature of control of work of others, exert no real influence in salary determination. So far as their effects are concerned these two variables appear to be mere incidents of sampling.

After having made due allowance for the net effect of the other independent variables, variable (X_3) Availability and nature of guide lines, explains mathematically 12.9 per cent of the hitherto unaccounted for variability still remaining in the dependent variable. In similar fashion, variable (X_4) Supervisory control exercised over the position, explains 11.0 per cent of the previously unaccounted for variability; and so on, for the other coefficients of partial determination, same order.

A Period of Great Growth and Development. 1926–1946. HOWARD WHIPPLE GREEN.

The eight largest cities were first laid out in census tracts for the 1910 Census. Only in New York City were census tracts put to work. In 1926, Cleveland and Chicago became interested in obtaining data from these earlier censuses by census tracts. The first census tract street index was created in Cleveland in 1926. Other cities became interested and by the 1930 Census there were 18 census tract cities. At that time communities interested in tract data had to reimburse the Bureau of the Census for transcribing the tables showing age, sex, country of birth, rents and values, and so forth. In April 1931, William F. Ogburn, President of the American Statistical Association, created the Committee on Census Enumeration Areas, and the Bureau of Census soon gave it an official assignment to promote the creation of census tracts in the largest cities throughout the country. In December 1931, the first census tract session was held at the A.S.A. meeting in Washington, D. C. Such meetings have continued to be held annually during the past 25 years. At the Cincinnati meeting in December 1932, the purpose was stated as promoting the use of census tracts through the exchange of ideas and experiences. As the years passed, new approaches and new motivation have been adopted. The tracted communities increased to 64 by 1940. In that year the Bureau of the Census published a bulletin for each tracted community, areas beyond the central cities were tracted, and smaller communities were encouraged to use census tracts.

The Economic Censuses. HOWARD GRIEVES.

The most conspicuous results of the 1954 Economic Censuses are listed as revolutionary changes in methodology, reduction in cost, shortening of the time period involved in compilation and publication, and new measures of economic activity. A brief progress report is given. Tabulations have been provided in various levels of detail ranging from national and regional totals to figures for towns and villages as small as 2,500 population and preliminary and final reports covering well over 5,000 pages have been issued. New coverage, new tabulations or arrangements of familiar census measures, and other changes are described in the paper. Among the items given specific attention are: Renewal of the Census of Mineral Industries after 15 years. Addition of public warehouses to the Census of Business. Coverage of Central Administrative Offices and Auxiliaries. Industrial Water Use by Manufacturers and Mineral Industries. Resumption of report of installed power equipment. Partial resumption of data on distribution of manufacturers sales. New tabulations covering company statistics, enterprises and their establishments, enterprise data of other organizations, concentration tables, central business districts, and other items.

A Theory of Financial Development in the United States. JOHN G. GURLEY and E. S. SHAW.

This paper attempts to explain the growth of primary securities, the money supply, and other financial assets in the United States from 1800 to the present. It utilizes financial data covering this period.

The growth of primary securities—the debt and equity issues of the nonfinancial sectors—relative to income growth is explained by the upward trends in the ratio of deficits to income and in the proportion of deficits financed by security issues. These two trends rest on the behavior of the long-run saving ratio, the growth in the division of labor between savers and investors, the development of financial markets, and the slowing down in the rotation of deficits and surpluses among sectors of the economy.

At interest rates compatible with a satisfactory rate of output growth, spending units will not generally demand the entire new supply of primary securities forthcoming year after year. The portion of primary securities rejected by spending units at these interest rates must be sold to financial intermediaries, including the monetary system. These intermediaries create indirect financial assets, monetary and nonmonetary, for spending units to hold as alternatives to primary securities. If nonmonetary indirect assets are to any extent substitutes for money, the demand for them affects the demand for money. The growth in the demand for money, therefore, is affected by all of the factors mentioned above, plus the transactions demand for money.

From Guaranteed Annual Wages (GAW) to Supplementary Unemployment Benefits (SUB). SEYMOUR E. HARRIS.

Although Mr. Reuther did not achieve all he hoped for, his campaign for a GAW dramatized the inadequacies in the treatment of unemployment. It might have cost industry twice as much to finance Mr. Reuther's program as their current commitments on GAW or better SUB. From the agreements with the auto companies, the trade unions have begun to build—e.g., the Allis-Chalmers and the Steel agreements. When SUB has been accepted, contributions to unemployment compensation are back at the rate prior to attrition through experience rating, that is, offer of low rates to employees with good employment records.

The fear of a GAW has not resulted in any significant improvement in the ratio of benefits under unemployment compensation (U. C.) in relation to weekly wages. State legislatures are still frightened of increasing costs and of losing business to competing states. As great as is the need of diverting part of the gains of increased productivity to taking care of the unemployed, an even greater case can be made for spending more for health and sickness insurance and old age. Disability insurance has bogged down. Comprehensive medical insurance is avilable to finance only a few per cent of all medical outlays. The old live on the average 12 years beyond 65 and are retired for about 6–7 years, or twice as long as in 1900; and they live on half the income of the others. But unemployment is only ½ the level of the thirties, and on the average the unemployed remains unemployed for about 10 weeks. Indeed the unemployed lose two thirds of their income when unemployed.

SUB makes a small, if not outstanding, contribution towards stabilization—on the assumption of coverage of 6 million, it might in a bad year contribute as much to stabilization as U. C. did in 1954 through the rise of disbursements. SUB also, since it puts greater burdens on states with ungenerous programs of U. C., puts roughly twice as great a burden on Southern as the Northern Industrial states and therefore tends to discourage migration to the south.

Critical Values for Duncan's New Multiple Range Test. H. LEON HARTER.

David B. Duncan (*Biometrics*, Vol. 11 (1955), pp. 1–42) has formulated a new multiple range test making use of special protection levels based upon degrees of freedom. Duncan (*ibid.*, Tables II and III) has also tabulated the critical values (significant studentized ranges) for 5% and 1% level new multiple range tests for various combinations of p and ν, where p is the number of ordered means in the subgroup being tested and ν is the number of degrees of freedom. The values for $2 \leq p \leq 20$ and $10 \leq \nu \leq \infty$ were obtained from tables by Pearson and Hartley (*Biometrika*, Vol. 33 (1943), pp. 89–99), and the other values from tables by Beyer (M.S. thesis, Virginia Polytechnic Institute, 1953). Unfortunately the approximation which Pearson and Hartley used in computing their tabular values results in serious errors for ν less than about 20, and the modification made by Beyer does not remove this difficulty. The purpose of the present paper is to report corrected values which have been computed by a different method. This method consists in the determination of the probability integral of the studentized range by numerical methods, followed by inverse interpolation.

Experiences with Univac Processing of Questionnaire Data. R. K. HARTER.

This paper describes the routine use made of Univac in the standard processing of Bell System Employee Attitude Surveys. The standard program is outlined. Reasons are given for the choice of Univac over alternative systems. Five problems faced in programming are discussed: coding the input, pre-sorting, combining differential samples, computing tests of significance and their confidence levels, and checking results. The problems of programming are mentioned briefly along with a few things that might have been done differently were foresight as good as hindsight. A brief evaluation of the success of the venture is offered. Univac has been used for three years with a 110-item questionnaire scored on 17 Guttman-type attitude scales. About 100 pages of statistical data are produced for each of 70 areas in a typical survey. For a typical Bell System Company 3-day service is an obtainable objective when the input is properly prepared.

Some Aspects of Statistical Research in Industry. ROBERT HOOKE.

Statistical research in industry has been restricted by lack of experience with industrial problems, by the shortage of statisticians, and by complacency resulting from success in applying known techniques. These restrictions must be removed if the industrial statistician is to approach his maximum usefulness. A particularly important area is the experimental investigation of functions of quantitative variables. Box and his associates have pointed out the now obvious fact that a chemist can study such a function sequentially, and that he may care for only one feature, say the location of the maximum. For him, a factorial experiment, completely planned in advance, is clearly wasteful.

Opinions differ on the usefulness of recently developed methods of maximum finding. The method of steepest ascent has obvious shortcomings, but the real deficiency is the absence of fundamental theory—e.g., there is no way to establish the superiority, or partial superiority, of a possible alternative procedure. Basic research, leading probably to a useful (mathematically and experimentally) classification of functions, is needed.

The industrial statistician, perhaps too busy to do research in this and other directions, should at least use his experience to point out directions for research and encourage his company to sponsor research in these directions.

Moneyflows and Monetary Theory. GEORGE HORWICH.

The paper describes the "Flow of funds" accounts developed by the Federal Reserve system. Two criticisms of these accounts are advanced: (1) A sharp distinction between flows of cash and other flows should have been made. The "Fed" confounds monetary with certain "accrual" or barter transactions which, from the viewpoint of monetary theory, should have been separated. (2) The grouping of transactions into financial-non-financial categories has little economic significance; a superior classification would be between transactions in assets (financial and otherwise) and all other monetary transactions. The use of the Federal Reserve methodology by the Postwar Capital Markets project of the National Bureau of Economic Research is also discussed. The Bureau is deriving highly detailed financial flows on a quarterly basis for the period 1953-55. The quarterly data promises to be the basis for the study of short-run monetary disturbances. For example, the impact of a Federal Reserve purchase of Treasury bills can be traced through the accounts, if supplemented with interest-rate data. One could study the intersectoral movement of cash, both financial and nonfinancial, stimulated by the bill purchase. In general, one should be able to isolate the mechanism by which disturbances in the money market are transmitted to aggregate demand and the level of employment.

Cyclical Changes in Costs, Prices, and Profit Margins. THOR HULTGREN.

Although managers of factories have reduced the amount of labor needed to turn out their products in almost every year since the war, the reductions have not been reflected in labor cost. Changes in wage rates and other factors have caused average hourly earnings to rise faster than productivity. Labor cost per unit of product increased in every year of rising prosperity except 1949–50. It fell a little in the 1948–49 and 1953–54 recessions. Before the war also, from 1919 to 1941, manhours required per unit of product usually declined from year to year. In that era, the decline was often translated into a decline in labor cost. When output was rising, total cost per unit including labor, materials and overhead, rose in about half of the years studied, fell in the other half. When output was falling, manufacturers of durable goods usually had rising costs, but in other manufacturing the changes in cost were not consistent in direction.

When prosperity is growing, manufacturers with falling costs are usually able to keep part of the benefit. The prices they get for their products rise, or at least do not fall in proportion to their costs. Even factories with rising costs are sometimes able to widen their margins, but not nearly as often. During a recession, on the other hand, even manufacturers with falling costs are usually unable to increase their profits per unit of product. The prices they receive fall faster than their costs.

The conclusions are based on averages for all manufacturing. The experience of individual industries sometimes departs from the general pattern.

Measurements and Designs in Assays of Immunizing Agents. JOHANNES IPSEN, JR.

Measurements of immunity in immunized subjects are obtained either by titrating blood samples for specific antibody or by recording reactions on the subjects following challenge with live agents on toxic products. The latter method is usually recorded as quantal response, with death of the animal as threshold marker. Use of high challenge doses and recording of survival time yields data that can be transformed to appropriate, graded scores. The "death score" measures over as broad an antigen dose range as the antibody titer and its low costs outweighs the higher precision of the antibody measurements.

Response curves to one injection of antigen tend to curve asymptotically for high doses. A method is suggested to utilize the ratio of the linear regression interaction over the quadratic regression as an additional estimate of potency ratio of two preparations. Two designs for assays using two subsequent antigen doses (secondary stimulus) are discussed, and their relative efficiency evaluated.

Some Notes on the Calculation of Death Rates for Establishments. A. J. JAFFE and CAROLYN SHILLING.

How can the death rate—or the rate at which establishments go out of business—be calculated in the absence of vital statistics? This question is particularly relevant for underdeveloped countries, which generally have few data.

Death rates and life tables for humans have been calculated indirectly from statistics in two consecutive population censuses which show the distribution of the population by age and sex. This general procedure was applied to the census of manufactures data for Colombia and Puerto Rico, and death rates by industry calculated in both areas. In each case the later census showed the number of establishments in operation classified by the year in which the factory was started, i.e., by age. By comparing the numbers by age, with the numbers of factories as reported in the earlier census, the number which have discontinued operations—or died—during the inter-censal period is obtained.

For Puerto Rico additional data were obtained, namely the list of factories which had been in existence at the time of the 1952 census of manufactures but had been discontinued by the time of the 1954 census. For each of these deaths the year in which the factory was started was obtained from the 1952 census return. By relating this information to the age distribution of all factories in 1952 age specific death rates (q_x values) were calculated. These q_x values were then converted into l_x values. Thus it was estimated that about 16% of all factories go out of business during the first year of life, and that not more than 30% survive 12 years, i.e., to age 12.

Additional examples are presented of the kinds of information about factories which can be obtained by fuller statistical treatment of the original census schedules.

Outlook for the Chemical Industry. JEREMY C. JENKS.

Competition and Confidence. These are the words that characterize the chemical industry today.

The principal result of the feeling of confidence that exists in the chemical industry is an aggressive expansion of physical facilities. This is also the cause of the intensifying competitive conditions that exist. In 1955 the chemical industry spent approximately $1 billion on new plants and equipment. A recent estimate for this year is moderately over $1.4 billion and a preliminary estimate for 1957 is slightly under $1.6 billion. These are record-breaking figures.

Since the Fall of 1955 the industry has been operating fairly near to effective capacity in many product lines. For this reason one cannot expect startling sales gains in many individual products such as occurred last year.

The chemical industry is in a healthy condition with operations at a high level. Managements are optimistic and are pushing their expansion programs aggressively, and as most of these can be financed internally, the tight money picture has not yet had any serious effect. Competition, however, is increasing and this is being reflected in profit margins. Chemical stock prices are high by historic standards and chemical stocks have in the last several years gone along fairly evenly with the rest of the market.

Yet the industry has lost none of its internal dynamics and the long pull investors in well selected chemicals will continue to reap profits.

Investment Forecasts. D. M. KREZER.

I have found it increasingly easy to see that business is going to continue to go along at a very high level over the months ahead. In fact, if I were disposed to worry about anything over the short run it would be the danger that we shall have a dizzy, speculative spree, with the inevitably painful hangover to follow. A high and generally rising level of business investment in new plant and equipment has provided the backbone of our long-sustained period of postwar prosperity. It will continue to provide the backbone of a high and quite steady level of prosperity in the years immediately ahead. Business investment in new producing facilities will be substantially higher in 1957 than it will be this all-time record-breaking year. And, with only a minor lapse here and there, it will keep going up right through the 50's. If during the next decade we are to equal the increase in output per capita we have made during the past decade our nation's total output must be increased by 35 per cent, with only 9 per cent more man-hours to do it. This is simply another way of saying that we must step up our investment in new and better capital equipment, both relatively and absolutely. American industry is spending about $5.5 billion for research and development (about 40 per cent of it financed by the federal government), and has plans to increase its expenditure to $6.3 billion in 1959. From this tremendous investment in research and development flow two consequences of major importance for capital investment. One is a flood of new products which create their own demand for the equipment with which to produce them. The other is a flood of new and improved processes and equipment. When coupled with competition, they also eventuate in greatly increased demand for capital investment. In addition to having an increasing need of capital investment by business, we now have new institutional equipment investment expanding at a relatively steady rate. This equipment has been provided by the sweeping expansion of long range planning of business investment in the last decade. In our office at McGraw-Hill we have been making surveys of plans for business investment in new producing facilities for the past nine years. When we started not over a handful of the cooperating companies could give us estimates of their expenditures for new producing facilities running beyond the current year. This year about 90 per cent of the companies sharing in our survey—and a far larger number of companies than was covered by the survey nine years ago—could give us estimates of the investment in new plant and equipment running three years ahead. Also this year, nearly a third of the cooperating companies indicated that their peak year of investment would come one or more years hence. In fact, almost 10 per cent of them said they would not hit their peak of investment until 1959. Nothing like this has ever happened before. As I see it, we are embarked upon a great historic surge of capitalist development which has nowhere near run its course. And seeing it that way, I prefer to proclaim it rather than to sit shivering awaiting reenactment of the great depression. This I believe is as remote from current economic realities and prospects as the Middle Ages.

Techniques and Uses of Forecasts of General Business Conditions in Eastman Kodak Company. EDMUND R. KING.

Successful use of economic forecasting in a business firm is possible only if management believes in a systematic program of forward planning. At Eastman Kodak, economic forecasts are intended to assist management in adapting company operations to the changing economic environment. In short-term forecasting (6 months to 2 years), both the sensitive indicators and the Gross National Product components approaches are used. The GNP components also provide a framework for the long-term forecast (2 to 5 years) but this interval is between the cycle and the trend, as normally defined, and the forecaster must rely on personal judgement rather than any standard techniques. Very-long-term forecasts (beyond 5 years) are worked out from trend analyses of aggregates and also from projection of GNP demand segments and input factors. Twice each year, an economic forecast for 5 years ahead is prepared in terms of specific economic indicators. After approval by top management, this economic forecast is used, along with much additional information, in the preparation of sales forecasts. These sales forecasts are a key element in the 5-year budgets and are also used for production scheduling and financial planning. A top management committee meets monthly to review the sales forecasts for the current year. At this meeting, a brief discussion of the current business situation is presented. This monthly economic presentation together with the regular 5-year forecasts prepared twice each year ensure that an up-to-date economic forecast is continuously available to top management and to other personnel involved in sales forecasting and planning activities.

Measuring the Potentials of Growth in the Great Lakes Area Following the Completion of the St. Lawrence Seaway. ROBERT C. KING.

Measurement of the growth potentials of the Great Lakes area following completion of the deep-draft St. Lawrence Seaway necessarily must take into consideration that the Maritime Administration is authorized, subject to specific requirements of the Merchant Marine Act, 1936, as amended, to:

(1) defray a portion of the cost of construction of U. S. flag ships built in U. S. Great Lakes yards (or other U. S. shipyards) for bulk cargo or liner operation between U. S. Great Lakes ports on the one hand and overseas foreign ports as well as Atlantic Canada ports;

(2) pay operating subsidies to U. S. flag ships that maintain liner service on Trade Route No. 32 between U. S. Great Lakes ports and Western Europe; and

(3) provide ship mortgage insurance (100 per cent of up to 87½ per cent of the cost of construction or reconstruction) on vessels including but not limited to trainships, trailerships, container ships, tankers and various combinations of bulk and package freight ships, owned by U. S. citizens for operation between the U. S. Great Lakes ports or between such ports and other U. S. Domestic ports.

Additionally, a recent amendment to the Merchant Marine Act assists privately-owned U. S. flag ships in obtaining approximately one-half of the U. S. Government cargo that moves in U. S. foreign trade in commercial ships, including large amounts of coal and grain that move under programs of the International Cooperation Administration and the U. S. Department of Agriculture. Furthermore, a substantial portion of the U. S. military cargo that moves under the direction of the Military Sea Transportation Service is carried in privately owned and operated U. S. flag ships. With the completion of the St. Lawrence Seaway, cargo of the above described categories will be available for movement between U. S. Great Lakes ports and many foreign destinations.

Other shipping factors that will affect the growth potential of the Great Lakes area but which cannot be measured at present include the amount of tolls to be levied on vessels and cargoes passing through the Seaway, the curtailment of Great Lakes shipping during the winter months, and the extent of coordination of Atlantic seaboard and Great Lakes shipping operations and services on many overseas routes.

Unsolved Problems in the Statistics of Survey Sampling. LESLIE KISH.

Much research, especially in the social sciences, is done by means of complex sample designs. Important populations are usually so large and widespread that simple random sampling (s.r.s.) would be uneconomical. But standard statistics have been developed in terms of s.r.s. The literature of complex sample designs deals only with means (and proportions) and corresponding totals and with the difference of two of these. There is great, unfilled need for the mathematical bases of analytical statistics for data originating in complex sample designs. Generally these analytical statistics are either not computed or they are computed incorrectly under s.r.s. assumptions. These result in gross mistakes because of the effects of "clustering" on the sample. Confidence coefficients may be distorted from $P = .99$ or $P = .95$ to $P = .50$! These problems need the urgent attention of mathematical statisticians, particularly to provide formulas valid under complex clustered designs for some of the most useful statistics; for example: 1) the coefficients of multivariate analysis; 2) analysis of variance and multiple comparisons; 3) some non-parametric tests. Recent advances in theory and in computational facilities provide fruitful backgrounds for research.

A Study of Political Attitudes and Behavior of Union Members. ARTHUR KORNHAUSER.

This is a study of UAW members in the Detroit area at the time of the 1952 presidential election. Both in their voting and in their more general political orientation, the auto workers were found to be predominantly in accord with the union's official position. In spite of their rapid rise toward comfortable middle class planes of living, most of them held to New Deal-Fair Deal and pro-labor views.

According to their prevailing sentiments, they approve union political activities; they trust labor's voting recommendations and the great majority cast their ballots accordingly; they are inclined to distrust the recommendations of business groups and newspapers; they want organized labor to have a larger voice in government and business to have less influence; they are overwhelmingly democratic and they look upon the Democratic party as the party that protects and advances the interests of working people; they identify themselves as members of the "working class" rather than the middle class.

On the negative side, however, a significant minority of the members feel and vote differently. Moreover, considerable numbers have feelings of political futility and indifference, personal dissatisfaction with life, low morale, and authoritarian attitudes, all of which affect their political behavior.

Those members who are *most* pro-labor in political outlook and also most politically aroused are found to be above average in education, skill, and income and to include a disproportionate number of younger workers, Catholics, Negroes, and American-born whites. These findings raise serious question about the idea that trends in the working population are making for reduced loyalty to unions and for more conservative political views.

Organization and Supervision of the Programming of Research Computations. CARL F. KOSSACK.

The emphasis given in the discussion of organization and supervision is to a medium size digital computer installation such as the Datatron Computer of Purdue University. It is recommended that the computer installation have as its primary objective that of serving research computations of the University. That such programming will be needed for some time in the future seems indicated since research workers are rarely satisfied with standard routines available in the library and the development of automatic translation and compiler procedures is still in the future. The Purdue programming effort is organized around three types of individuals: 1) the professional programmer, whose task is to program on a full-time basis; 2) the associate programmer, whose task is to program particular research problems within special areas, and 3) the amateur programmer whose task is to advise the other two types of programmers on the requirements of their particular research problems.

The programming effort is evolved on a decentralized basis with each division of the University making some effort to develop its own programming capability. The Central Laboratory assists this effort by providing consultants and giving short courses on programming. There is a distinct advantage to having the computer installation associated with a statistical laboratory since problems that arise require more than routine computational advice. The types of statistical routine now available for the Purdue Computer include linear regression analysis, analysis of variance routines, empirical distribution generation, random number generation, factor analysis, and maximum likelihood routines.

Expenditure-Income Relationships for Consumers Durable Goods and Problems in Their Derivation. IRVING B. KRAVIS.

This analysis of expenditure-income relationships for consumers durable goods is part of a broader investigation of consumption patterns being carried on as part of the Wharton School Study of Consumer Expenditures, Incomes and Savings. The Wharton study is based largely on the 1950 survey of urban families by the Bureau of Labor Statistics, but earlier BLS urban surveys are also drawn upon for purposes of the present paper.

On the basis of closeness of fit, economic rationale, and economy, the simple linear form was chosen as the most suitable for application to a wide range of categories of consumption. In analysing each category, total consumption rather than income was taken as the independent variable chiefly because the observed level of consumption is thought to provide a better approximation to permanent income status than does the observed level of income. The extreme income classes were generally omitted from the regressions because their expenditure-income relationships could not be described by the same simple regressions that were adequate to account for the behavior of the 85 to 90 per cent of the families and individuals in between the extremes. (The proportion of after-tax income and of consumption accounted for also fell within this range.) The loss of predictive value resulting from the omission of the top

income group is reduced when intertemporal comparisons can be made of the behavior of the great bulk of families in each of several surveys. In view of the exclusion of the extreme income classes where the problem of heteroscedasticity is greatest and in view of the uncertainty concerning the appropriate correction, it was decided to use only frequency weights and to forego any attempt to correct for heteroscedasticity.

Substantive results are presented for 5 important categories of durables for 1950, 1941, 1935–36 and 1918. The marginal propensities for 1950 were highest even after allowance is made for the impact of Korea. The average propensity to consumer durables has shown an upward trend and was highest in 1950 both in terms of given money and given real incomes. The post-World War II rise in the durables marginal propensities reversed what was apparently a downward trend in durables elasticities.

Relation of Michigan Personal Income and Gross National Product. DICK A. LEABO.

The objective of this paper is to study the relationship of the level of Michigan's personal income and the value of the goods and services produced by the nation's economy, Gross National Product. This paper then deals primarily with determining the effects of changes in the level of GNP upon the level of personal income in Michigan. In addition, the relationship between Michigan personal income and consumption expenditures for automobiles and parts, and the relation of Michigan personal income and National Income originating in the automobile industry are studied. The slope of the regression line, as measured by the coefficient of regression, is $39.2 million. This means that for every $1 *billion* change in GNP, Michigan's personal income tends to change at the rate of $39.2 *million*.

Relative to the output of the nation's goods and services, Michigan personal income has shown a slightly rising trend. On the average, total personal income in Michigan tends to increase by more than $1.3 million per year *if* all other factors remain constant.

Estimates of personal income by state compiled by the National Income Division of the United States Department of Commerce generally are not available until at least eight months following the ending of any year, *e.g.*, the 1955 figures will be available in August or September, 1956. Therefore it may be desirable to attempt to forecast or predict the level of Michigan's personal income on the basis of a relationship similar to the one described above. By substituting in the regression equation and by analyzing what has happened to the economy in the nation as a whole, it is possible to indicate the probable level of a state's income months in advance of the release of the official data.

In making any forecast based on a relationship similar to the one described above, one may well ask "How good is this estimate?" The standard error of the estimate should be the basis for our answer. The standard error of the estimate, a measure of the disperion of the dots about the regression line "AB." was computed to be $252 million. For example, with a GNP of $387 billion in 1955 we would forecast Michigan's personal income to be approximately $14,777 million. The chances are two out of three that the actual income will fall within the range of $14,777 million plus and minus $252 million or within the $14,525 to $15,029 million range.

Machine Capability Study Through Analysis of Variance. M. M. LIGHTSTONE.

Machine capability in metal cutting operations can be effectively determined by identification, measurement and exclusion from total variation of a dimensional characteristic, that variation which is due to removable causes. Capability limits are then said to be 6σ, where σ^2 is the residual variance.

This paper applies analysis of variance to boring a hole in an automotive transmission part. Different machine cycles, chucks and locations within the part are evaluated for effect upon diameter size. Within piece variation in the form of an elliptical bore was found to be the principal problem.

Checking parts before and after release from the chuck indicated part distortion during machining by excessive, unbalanced chuck pressures. The analysis thus served to identify the major cause of trouble, measure its contribution to total and measure both present performance of the machine and what its capability would be following corrective action.

Experimental design in machine studies should anticipate and include factors such as variability within pieces; tooling and fixturing; multiple machines, stations and cuts; hardness and dimensional variation of rough stock. Application can be made to insure adequate performance of new equipment, critical evaluation of tolerances, limitation of scrap and rework costs and effective maintenance of old equipment.

Quality of Business Establishment Data in Commercial Surveys. BENJAMIN LIPSTEIN.

Response analysis cannot be carried out independent of the collection process. There are two sources of variation in the collection phase of a survey: (1) due to collection mechanism being used and (2) due to the variation of respondent answers. An adequate attack on the response problem calls for (1) an evaluation of the quality of execution of the given mechanism and (2) an evaluation of the quality of respondent response under the given mechanism. The paper draws a distinction between a mechanism value—a value which the given collection procedure will elicit and the true value. A quality control procedure of interviewers is examined as a means of evaluating the variability of the mechanism value—the consistency and quality level of execution of the collection mechanism. Comparisons of estimated aggregates from the survey against known totals is the second technique considered for evaluating the collection mechanism and response. Lastly, dual collection methods are considered for evaluating these two problems. Some actual experiences in commercial research are used to amplify the theoretical framework.

Statistics and the Predictability of Weather. ROBERT G. MILLER.

The amount of predictive information in an observational network has not been explicitly determined for the case where the number of observed variables is large. From a statistical standpoint, one of the reasons for this is the sheer magnitude of the task of solving for the prediction operator when the number of predictors reaches one

hundred or more. Generally, an approximation scheme is used either to specify mathematically the large number of variables by means of a few parameters or to select a few of the total number of variables on physical grounds. Through the application of a new screening procedure in multiple regression analysis it is now possible to process many more predictors than could be done previously. The feature of this technique is that from a large set of possible predictors a small number can be selected which contain practically all the information of the entire set concerning a specific predictand. An experiment was conducted using this method to investigate the predictability of certain weather elements. The predictors for this experiment consisted of seven weather elements measured at each of forty-eight stations scattered over the United States. Results of the experiment will be presented, as well as tests on independent data. The potentialities of such a procedure will be discussed in the light of present day high-speed computing methods.

The Business Outlook and the Liquidity Squeeze. GEORGE W. MITCHELL.

A record year for business activity in 1956 was predicted. Employment, total factory output, retail trade, business capital expenditures, and total construction will all head for new highs. The strong liquidity position of the immediate post war years has been substantially cut down in all sectors of the private economy in recent years. From 1945 to date, business has spent about 250 billion dollars on new plant and equipment. More than 11 million new homes have been built. Consumers have purchased about 50 million new cars and a multitude of other durables. State and local governments have been clearing off their shelves of public works while raising their sights on the quality and quantity of community facilities appropriate to the "new era." Competition for a limited supply of funds has seldom been so intense as during the current year. Already the pressure on liquidity has caused some planning goals to be postponed or reduced in scope. In 1957 the spotlight will be focused on the nation's savers and investors and its monetary system more sharply than ever before. Current evidence indicates that capital formation is now proceeding at a faster rate than the economy can support without threatening the stability of the dollar. In the past year especially, financing this rate of outlay has brought about a considerable deterioration in the liquidity position of consumers and businesses. Throughout the entire private sector we cannot look to further decreases in liquidity to supplement the flow of savings as it has in 1956. Now that individuals, businesses, commercial banks, and other financial institutions have strained their own resources to the maximum, attention may be increasingly directed toward the lender of last resort—The Federal Reserve System.

If monetary policy is to work smoothly and effectively, borrowers and lenders must recognize the symptoms of inflation and be prepared to share the responsibility for remedial measures. At the moment it is too early to know whether consciousness of this responsibility, an intuitive desire for liquidity, or more aggressive action of the monetary authorities will pay the major role in the coming months.

The Outlook for Stock Prices. M. DUTTON MOREHOUSE.

The variables involved are such that stock market forecasting defies statistical analysis. Intuition is the touchstone of success, and stock market forecasting is definitely an art, not a science. Fundamentally, common stock values are dependent upon the capitalization of present and all future dividends, which in turn are dependent upon the return on invested capital. The multiplier or capitalization of earnings and dividends at any given time will vary with people's financial position or their ability and desire to buy. One can forecast a higher level of stock prices with considerable assurance if one looks far enough into the future. Few of us, however, can or will be satisfied with such a long look and one cannot view the more immediate future with the same degree of equanimity.

Since the end of World War II, stock prices have been supported by increased earnings on invested capital and by an increase in the multiplier induced by the Eisenhower confidence boom which began in the fall of 1953. In contrast, profit margins have in general declined. The postwar increase in earning power and in potential dividend payout has clearly been based on higher sales volume. Those companies which have been most successful in recent years in maintaining profit margins are those which have been able to replace labor with low cost capital investment. Construction costs have more than doubled since the end of World War II. The cost of developing new sources of raw materials is likewise increasing. The ability of manufacturing industry to substitute low cost capital for high cost labor may thus lose its effectiveness in maintaining profit margins.

The trend towards automatic escalator clauses in wage contracts and a decline in the adult population in the labor force due to the low birth rate of the thirties should produce a continuation of the tendency to increase labor costs more rapidly than per man hour productivity. In the longer run, costs undoubtedly do govern selling price, but in the short run in our competitive economy it is demand, not cost, which establishes selling prices. During the past year we have had ample evidence of consumer resistance to higher prices as markets have been more plentifully supplied with goods. In the intermediate future, this cost inflation could well result in a cost price squeeze on operating margins sufficient to overcome any psychological factors operating to maintain stock buying as an inflation hedge.

Capital expenditure programs can be expected to continue at their present high level with perhaps some stretchout due to financing problems, but it is hard to see any further expansion in this field. Should consumer demand for the end product prove disappointing, a more pronounced stretchout or postponement of these plans is likely.

Essentially, we are left with the consumer as a prime factor in determining the business outlook. Although consumer incomes will be high during the fourth quarter, I am inclined to believe that we may find considerable reluctance to pay the higher prices which manufacturers are rather generally announcing for their end product.

Monetary policy designed to curb rising prices has driven the yield on high grade corporate bonds to 4% or better. The institutional buyer today has a place to go with his money other than the stock market. Under such circumstances, I do not believe that a decline in stock prices will find the support from the institutional buyer which we have come to regard as a perpetual stabilising influence on the market.

Forecasting Plant and Equipment Expenditures from Businessmen's Expectations. VITO NATRELLA.

This paper attempts to evaluate the results obtained in the regular series on business plant and equipment expenditures published jointly by the Securities and Exchange Commission and the Department of Commerce. Analysis of the aggregate estimates and of the individual company data as to accuracy of anticipations gave the following results: In general, the annual estimates based on anticipations as published have accurately forecast plant and equipment expenditures both as to level and movement. With one exception, turning points were clearly indicated. The seasonally adjusted quarterly data also indicate a high degree of accuracy. This accuracy in both quarterly and annual expectational data reflects the advanced planning involved in business decisions and the tendency for influences modifying these decisions to offset each other in aggregate. Anticipatory data appeared to provide better estimates of capital outlays than did projections of past or current data. This was indicated both on an aggregate basis and for individual company data. Positive and negative discrepancies in anticipations for individual companies tended to offset each other. However, there was considerable variation in the degree of dispersion. Accuracy appeared to be closely related to industry and size of company and to the relative magnitude of the investment program.

There appeared to be a systematic tendency on the part of manufacturing firms to underestimate capital outlays for plant and equipment, This was closely related to size of company, the larger companies showing little or no tendency in this direction. For the period studied, there was little evidence of any close over-all relation between accuracy and changes in company sales or between accuracy in one year and accuracy in the preceding year.

Criteria for Evaluating Consumer and Real Estate Credit. ROBINSON NEWCOMB.

The thesis of the discussion on criteria for real estate credit is basically the argument that the security behind real estate paper can be judged best in the light of probable trends in real income and its distribution, and the propensity to spend this income for housing.

Real income can be estimated only if allowance is made for the income of a second earner, usually a wife. The tradition of ignoring incomes earned by other than the principal wage earner was defensible during the depression, but, like many other depression-born traditions, this needs to be carefully examined.

The common practice of basing values for mortgage purposes on reproduction costs, except for new buildings is a conceptual monstrosity. Current market value and capitalized rental value plus capitalized so-called ownership amenities made more sense in a buyers' market in a depression than it does in a high employment expanding economy framework. Houses designed so that they can be adapted to the demands of a rising income economy may offer a much better security ten years hence than houses not so designed though their current market appeal may be no better than houses with traditional design.

The claim that the introduction allowances for trends in prices, income, the relationship of cost to income, and propensity to use income for housing purposes introduces a needless judgment factor and one which cannot be defended is not true. Assumptions have to be made as to future securities under any system. The system suggested in this discussion merely recognizes rather than ignores facts.

Finally, criteria for real estate credit should be based on the assumption of general prosperity. Real estate credit itself cannot protect against depressions. If it attempts to do so, it would increase the likelihood of depression and decrease the security it purports to defend.

The Introductory Course in Statistics for Sociologists. DANIEL O. PRICE.

Some of the purposes of the introductory course in statistics for sociologists are (1) to show the student the utility of statistics in sociology as a means of summarizing data, making estimates, and testing hypotheses, (2) to teach the student some of the fundamental statistical processes, not just as computing techniques but as ways of answering pertinent questions, (3) to show the student the relationship between sociological theory and statistical methods, and (4) to interest the student in learning more about statistics as a basic research tool.

It is thought that these purposes can usually be best served if the first course is taught in the department of sociology or department of statistics at the undergraduate level. A prerequisite of college algebra is about as much as can realistically be expected. The materials should be ordered so as to hold the student's interest as much as possible. Students must be taught not to use statistical methods blindly as statistical clerks but as tools to help answer significant theoretical questions.

Techniques and Uses of Forecasts of General Business Conditions in U. S. Steel. J. L. REID.

A forecast of general business conditions usually reflects the numerical expectations for a number of economic factors loosely defined as "general indicators." These may be subdivided into background factors which provide general information, but are not directly indicative of business activity; general indicators which reveal business conditions; and such intermediate and specific determinants of demand for steel products as output of passenger cars and machinery and business spending plans.

The function of a forecast of general business conditions is to provide background information for a specific market and sales forecast. In considering the business outlook for the short run (three to six months) we watch general business indicators very carefully on a monthly basis; and in our forecast for the coming calendar year, we always put together a general economic picture as well as an appraisal of the major steel consuming industries.

The utility of these various background factors and general indicators varies considerably according to the time span of the forecast. In long-range forecasting, factors like population, households, labor force, energy are very important in estimating the size of the economy. A forecast of general business is an invaluable prelude to a determination of long-range demand for a specific commodity such as steel, since it reduces the possibility of understating demand based on a summation of requirements of existing industries; and it serves as a balance to prevent estimates of individual industries from getting too much out of line. However, in order to develop information on product mix

or geographic location of new facilities, there is no substitute for a careful industry-by-industry forecast. The key to successful forecasting is the integration and balancing of the over-all and detailed approaches.

Growth of Manufacturing in Underdeveloped Areas. H. W. ROBINSON.

This paper presents a summary of some work that has been done in the Council for Economic and Industrial Research for the ICA in the process of producing a manual entitled "How to Select Dynamic Industrial Projects." Detailed exposition of the technique described below will be found in Chapter XIII of that Manual, which is being published by the International Cooperation Administration, Washington 25, D. C.

It was felt that it would be useful, especially for underdeveloped countries not familiar with these methods, to present a numerical example of the use of the inter-industry technique in developing an investment program. The numerical example shows, in actual figures, how one of the simpler inter-industry techniques can be used by the government to select a set of industrial development projects.

In the example, two periods of time are considered. They are assumed to be two-year periods. The first, 1955–57, is concerned with the problem of what capacities to build to achieve the objectives of the second, which covers the two years 1957–59. The problem is to select the best set of industrial projects for the years 1955–57.

All testing and manipulation of investment projects in 1955–57 is with the object of achieving a desired schedule of deliveries to consumers, exports, government, and investment referred to later as "final demand deliveries," in 1957–59. This schedule could be part of a "plan," but it could also be an economic forecast of what can be achieved as a maximum by the economy in 1957–59, and what would be the commodity composition of consumer demand and government expenditure in those years. It might, for instance, be the result of a detailed projection of growth based upon a gross national product analysis.

The example begins with the assumption that a provisional set of investment projects has been evolved on the basis of engineering and economic studies, and is to be tested for balance and feasibility. It is assumed that such studies have revealed that a sound principle on which to base an investment program in the country concerned is the replacement of imports of certain commodities by home production. The program consists, therefore, of the building of substantial capacity to produce three products, the requirements of which were formerly entirely met by imports, namely—textiles, fabricated structural steel products, and other fabricated metal products.

By examining how far the set of twenty sector-capacities which would result from execution of the program can supply *second period* requirements for domestic proudction, we find the program to be infeasible, in that some capacities are overexpanded and some capacities are not expanded enough to provide the required domestic production.

The next step is to try to develop a better program. We examine in more detail the second period, 1957–59, in order to have a better basis for determining how much new capacity must be built in the first period. We determine what capacities would be required in the second period to meet exactly the desired final demand deliveries in that period. The first period investment program is then generated by these second period capacity requirements, being merely the difference between them and the capacities in existence at the beginning of 1955–57. It includes, as well as the three new industries, an expansion of some old ones.

We now calculate the first period production levels required to achieve the first period schedule of deliveries to consumption, exports, government, and to the *generated* investment program. We find that, in this period, required production levels for items that cannot be imported are within capacities, but that we would require more foreign exchange than is available.

We next test the introduction of only two of the three new industries into the country, continuing to import all requirements of products of the third industry (that with the smallest comparative advantage). We re-calculate the required set of second period domestic production levels, and discover that the introduction of the two industries into the country is enough to ensure, in the second period, capacities to meet production levels and no foreign exchange shortage. Upon re-calculating the required first period investment program, and the first period production levels, we find that, in that period also, capacities are adequate and that foreign exchange requirements are *now* feasible.

We have, therefore, found a feasible program. It consists of introducing certain amounts of only two of the three industries into the country. It should be emphasized that the above feasible program is not the only feasible program, and is probably not the optimum program. Application of linear programming techniques (not used in this example) would enable us to arrive at the *optimum* program of development which would undoubtedly differ from the feasible program discovered by the above method.

Random Balance Experimental Designs. F. E. SATTERTHWAITE.

A random balance statistical design is one in which the n combination of the imput variables used are chosen at random from all possible combinations of the variables.

The advantage of this design is that *any* statistical analysis on any *a priori chosen* subset of the randomised variables *ignoring* all other imput variables is valid. Thus if we split the model *a priori* into the form,

$$Y = AF + BG + E$$

$$AF = a_0 + a_1 f_1(x_i) + \cdots + a_k f_k(x_i), \qquad k < n - 1$$

$$BG = b_0 g_1(z_i) + \cdots + b_\lambda g_\lambda(z_i), \qquad \lambda \gtrless n$$

$$X, Z \text{ independent}$$

then *any* statistical analysis to evaluate AF will be *valid* since

$$E' = BG + E$$

satisfies the necessary independence conditions. The b-unknowns can then be estimated by additional analyses on additional *a priori* splits. The total number of unknowns evaluated may exceed (and may vastly exceed) the number of data sets.

For efficiency, it is desirable to split the input variables into x and z groups by hindsight (feedback) instead of *a priori* so as to remove any terms from E' that have large effects. This requires selection rules that do not introduce "pooling error bias." Work to date indicates that efficient bias-free procedures exist though they have not yet been developed in publishable form.

Random balance designs (with feedback) approach Shannon Information Theory efficiencies when the numbers of unknowns and data sets are large (i.e. greater than 20), particularly when most of the terms in the model contribute only small amounts to the total variation in y. Analyses to evaluate 100 unknowns using only 20 data sets are routine. An analysis with respect to a model implying millions of unknowns has been successfully carried out with 250 data sets and with independent verification of the correctness of all conclusions.

Cycling. RICHARD SAVAGE.

Whenever a large quantity of property is under the management of a single organization, a well devised plan for maintaining the quality and keeping accurate account of the property can lead to substantial savings. The purpose of this paper is to present several applications of mathematics to the solution of these "cycling" problems. The formulations given here are of the simplest possible kind. The treatment presented does seem worthwhile, however, for it suggests the outlines of solutions to real problems and the feasibility of approaching more complicated problems of this type.

Several problems will be treated. The common element in these problems is that a solution consists in choosing a "program," i.e., a sequence of times, at which certain routine operations should be performed in order to minimize losses. The solutions presented will all be based on the assumption that certain costs functions are known. In some situations we will only need to make "geometric," e.g., concavity, assumptions about the costs functions. In other situations we will require the functional form or even the completely specified form of the costs functions. In applying the techniques of this paper the acquisition of this information might well be the principal difficulty.

The three main problems considered are concerned with the choice of optimal procedures in a replacement program, in an inventory control program, and in a preparedness program. In the replacement problem the solution involves the minimising of the long run average cost per unit time of "bank" replacements. The solution to the inventory problem is the program which will minimise the costs per year that arise from taking inventories and from imperfect information regarding the inventory. In the preparedness problem the solution program gives the procedure which will minimize the expected costs required to send equipment into the field when an emergency arises.

The Role of Between-Assay Error in Biological Assays. MINDEL C. SHEPS and PAUL L. MUNSEN.

Discrepancies in relative potency estimates within a biological assay method may be due to deviation from one or more of the assumptions implicit in the assay model. In a chick comb assay method for androgenic potency, observed variation between replicate M's for urine extracts had been three times as great as predicted. No systematic causes were apparent from a study of the data.

A change in technique that simplified the problems of handling the material to be applied to the comb resulted in only slight changes in the intra-assay variability, while the residual variance and assay slopes were still heterogeneous. However the observed variation of the M's for urine extracts now agreed very well with the intra-assay predictions. M's for steroids of varying chemical structure had an observed variance 1.7 times as great as predicted.

An experiment was performed consisting of 4 assays (replications) comparing the effects of 4 equally spaced log doses of androsterone (the reference standard), a urine extract (L25) and the steroid methyl testosterone (MT). In a combined analysis of variance the only significant interaction found was for assays by differences in the slope of MT as compared with the other two slopes. The observed variation in M's for both unknowns was not significantly different from that predicted.

The M's obtained with a parathyroid assay method were also not excessively variable despite the marked variation in residual s^2 and, in many of the assays, poorly determined slopes.

The present data as well as numerous reports in the literature suggest that inter assay variation, both in biological and chemical assay methods, can be detected only if specifically sought for. When present, it should be included in estimates of error. When it is due to a cause other than non-parallelism it is often possible to diminish is markedly or to eradicate it completely.

The Creative Manpower Shortage. ROCCO C. SICILIANO.

White collar workers now represent the biggest single group in the American labor force. For the first time in our history there are more white collar workers than blue collar workers. Included in the white collar group are the professional personnel, the managerial group and clerical and sales personnel.

An increase of twenty million people will occur in the American labor force by 1975. At that time about 37% of American workers will be found in the white collar group. Generally speaking there will be continuing technological advance which would accelerate the demand for creative manpower.

Creative manpower is "those individuals who, through their innovations, help us advance toward a higher standard of living and a higher level of national security. They exist and are needed at every level of our society across the occupational structure of the USA." The first half of this century witnessed a substantial increase in the skill level of American workers and this trend will continue through 1975. Under these circumstances, the big emphasis from now on out will be upon quality, upon creativeness, and upon leadership on the part of American workers.

Results of a Comparison of the Guttman Scaling Technique and Factor Analysis of Attitude Questions. P. J. SIEGMANN.

The Guttman scaling technique and a modification of the Thurstone multiple-group method of factor analysis are compared empirically as methods of analyzing attitude items. In practical applications of the Guttman scaling technique the investigator is frequently required to construct a number of different attitude scales. The Guttman technique does not provide a method for investigating the relationships between these scales. In addition, the Guttman scaling technique places restrictions on the items which enter into the analysis, the analysis being limited to those items within each scale which the investigator, on *a priori* grounds, considers fall in a defined area of content. A factor analysis procedure permits the analysis of all items simultaneously.

Ten previously constructed attitude scales were selected from an attitude questionnaire constructed using the Guttman technique. The fifty-eight items in these ten scales were analyzed by both techniques. A sample of 1,000 questionnaires were selected for this comparison. Application of the Guttman scaling technique validated the previous scaling. A factor analysis of the fifty-eight items provided a basis for comparing the two techniques. For two of the ten scales the two methods of analysis were in agreement. There were differences between the results obtained by factor analysis and the scaling technique on the remaining eight scales.

On the basis of the factor analysis, it seemed appropriate to redefine seven of the scales and regroup the items which were related to these scales. In addition to the scale factors, four additional factors were identified which were considered to account for the relationship between the scales.

Concepts and Response in Statistics on Business Establishments. WALT R. SIMMONS.

A statistical survey is viewed in the following terms: On the one side there is the statistician who seeks the answer to a problem through the collection of data that are measures of certain concepts. On the other side there is the respondent who interprets the statistician's question and provides data accordingly. There is and ought to be interaction between the formulation of concepts to be measured statistically, and the responses and capacity for response to a questionnaire.

This is the thesis which underlies a Response Analysis program in which the Bureau of Labor Statistics is studying employer reports in the Federal-State monthly statistics on employment, earnings and hours of work. One phase of this program is a quality-checking survey of 440 of the 40,000 plants which report data for manufacturing industries. The 440 plants are a 2-stage stratified probability sample of the parent panel. The first stage provides a sample of 61 of 587 geographic areas in the U. S. The second stage draws plants within selected areas. Over-all stratification took account of distance of area from home base of the nearest BLS agent, density of plants in an area, size of plant, and industry (the last being accomplished through systematic selection in the second stage). The survey was conducted through an intensive 200-question personal interview by professional staff members of the BLS.

Statistical results are being tabulated and studied, and will be published in 1957. BLS expects to strengthen reporting instructions, tighten editing, or alter choice of *concepts being measured*, as the evidence argues is most appropriate.

Interpretation of Regressions in Analysis of Covariance. H. FAIRFIELD SMITH.

This paper reviews the interpretation of regressions in analysis of covariance and of subsequently adjusted means, particularly with reference to cases where the "independent" variable is associated with or affected by treatments. It is pointed out that unless an independent variable has been deliberately varied one can seldom be sure of the causal effect underlying a regression, and that as a consequence customary inferences may often be dubious. Adjusted treatment means may be artifacts without tangible meaning.

The Horizon Production Scheduling Method. GIFFORD H. SIMONDS.

A new method, called the Horizon Production Scheduling Method, has been developed for scheduling the manufacture, transportation, and inventory of a seasonal product such as heating oil at minimum cost to meet an uncertain sales demand with a given high probability. A study of each class of sales such as retail, jobber, and cargo buyers, indicated an effect of weather and a general growth trend. A study of the sales demand data within any year indicated no significant time serial correlation and it was assumed therefore that the monthly sales are independent of each other. A study of the weather data composited for the marketing area in proportion to the present sales demand showed a long-range downward trend with time for each month. It is believed that the decrease in degree days per day noticed for every month may be part of a very long-range cyclic trend. However, the linear trend is highly significant over a fifty year period. No time serial correlation (persistence) was observed from month to month. The estimated variability in the estimated actual sales demand as variance is given to adequate accuracy by a propagation of errors technique.

The maximum sales demand for scheduling purposes can then be determined for a given level of risk of not meeting the peak sales demand.

$$Pr\ (S_j^* \geq S_j) = \alpha_j$$

where

S_j^* = maximum sales for scheduling purposes M bbls./day.
S_j = actual developing sales M bbls./day.
α_j = probability that $S_j^* \geq S_j$ (as a policy set $\alpha_j = 0.95$)

The scheduling method should provide adequate stock to meet the maximum sales demand defined as $S_j^* t_j$ in any period and should provide for cost minimization. Thus

$$I_{j-1} + R_j t_j \geq S_j^* t_j + I_{\min}$$

where

R_j = production rate M bbls./day in jth period.
I_{j-1} = Inventory (M bbls.) at start of jth period.
t_j = days in jth period.
$I_{\min}$ = minimum inventory to protect distribution system.

The horizon period for production scheduling is defined as that value of H which will maximize all R_j $(j = 1 \cdots H)$ subject to

$$I_0 + \sum_{j=1}^{H} R_j t_j = \sum_{j=1}^{H-1} \gamma_j \hat{S}_j t_j + S_H^* t_H + I_{\min}.$$

where

γ_j = economic factors depending upon the loss incurred when the estimate deviates from the actual sales.
$\hat{S}_j$ = estimate of sales demand in jth period M bbls./day.

In addition to the production cost, the product will incur transportation and inventory costs. The minimum total cost will be:

$$\mathcal{C} \underset{R}{=} \min. \sum_{j=1}^{H} [(C_j(R_j) + T_j)R_j + K_j \bar{I}_j] t_j$$

where

$\mathcal{C}$ = total cost $\$M$ for periods 1 to H to be minimized by selection of R_j.
$C_j(R_j)$ = average production cost \$/bbl. at rate R_j.
T_j = transportation cost \$/bbl. in period j.
K_j = inventory cost \$/bbl./day in period j.
$\bar{I}_j = I_{j-1} + I_j/2$ average inventory M bbls. in period j.

The solution is obtained by setting the partial derivatives of $\mathcal{C}$ with respect to $R_j t_j$ equal to a constant λ_H while meeting the constraint for total production in periods 1 to H.

Some Results of the 1954 Census of Agriculture. CONRAD TAEUBER.

The paper presents highlights of the 1954 Census of Agriculture, which provides new statistics about the agricultural enterprises in each of 3,000 counties in the United States. The census reflects the situation in a segment of the national economy which in 1954 had a gross value of sales of $24.6 billion, in addition to providing food, shelter and fuel for resident families on more than four and three-quarter million farms. The self-sufficient farm is virtually nonexistent today. In 1954, among the commercial farms three in five hired some labor, three in four bought feed for their livestock, four in five bought gas and oil, and seven in ten bought fertiliser. Part-time farms also reported expenditures for one or more of these items.

The number of farms has continued to decline, but there has been virtually no decline in total acres in farms or total acres cultivated. Despite consolidations, farm enterprises are small in comparison to most industrial enterprises. Generally they are operated by the farmer himself and only about 20,000 are operated for entrepeneurs by hired managers. The bulk of farm production comes from the two-thirds of the farms which are classified as commercial, and slightly more than a fourth of the total value of products was accounted for by the three per cent of commercial farms with gross income of $25,000 or more. More than half a million farms are part time farms, whose sales amounted to less than $1,200 a year and whose operators worked off the farm for 100 or more days during the year or whose family income from nonfarm sources exceeded the farm income. The average farm in 1954 was valued at nearly $20,000 for land and buildings alone, an increase of 150 per cent over 10 years earlier. Three in five commercial farms have tractors, nearly half of the farms have trucks, and seven in ten have automobiles. One in four commercial farms included in the 1954 Census had no power of their own—no tractors, horses, or mules. Three in eight commercial farms reported they had tractors, but no horses or mules. Thus, five in eight commercial farms had no horses or mules. The growth in electricity on farms has been rapid. In 1954, 93 per cent of all farms reported electricity while in 1920, 93 per cent did not have electricity.

The number of farms reporting some irrigation and the total land under irrigation is greater than ever before. About one-sixth of all crops were harvested from irrigated land in 1954. The use of practices for soil conservation has grown. In 1954, 435,000 farmers reported they used contour plowing on 22 million acres of land used for grain or row crops. Three-fourths of all farms are operated by their owners. Sharecroppers have decreased in number to about 273,000, approximately half the number reported in 1920 when the census first reported on this type of tenure.

Introduction to Finite Mathematics. G. L. THOMPSON.

Introduction to finite mathematics is a new kind of freshman course that has been taught by the mathematics department at Dartmouth College for the past two years. The basic part of the course consists of the following topics (taught in the stated order): the logic of compound statements, set theory, partitions and counting, probability theory (including Markov chains), and vectors and matrices. The idea of a set of logical possibilities (introduced in the first topic) and its subsets help motivate the work on set theory and partitions, and also gives a natural

way of defining probability. The study of Markov chains gives rise to (transition) matrices which helps to motivate the material on vectors and matrices. In addition to the above topics some of the following can be included: linear programming, game theory, and certain other mathematical models. Throughout the course the useful pedagogical device of a "tree" helps in explaining concepts. The material of this course, together with a semester of calculus is in the spirit of the recommendations of the Duren committee on the revision of the undergraduate mathematics curriculum, and also agrees with the time distribution on the various topics as recommended by the Madow committee on the mathematical training of behavioral scientists. There are no college prerequisites for the course but 2½ years or more of high school mathematics is desirable. It can be used to supplement or replace the traditional college algebra course and would fit well as a pre-statistics course.

Graphical Analysis of Two Level Factorial Experiments: A Decision Procedure. H. MACK TRUAX.

A new approach to analysis of two level factorial experiments is presented. Estimates of effects are obtained by the method of Yates. The absolute values of these effects are ranked in increasing order. A decision procedure is developed for testing the p largest absolute ranked effects. Here $2p-q=n+1$ where p is the number of main effects and q the degree of fractionation.

The decision procedure first tests the null hypothesis that all effects have mean zero against the alternative, some effects have means not equal to zero. Rejection of the null hypothesis for all effects requires additional tests of hypotheses on decreasing rankings until a null hypothesis is accepted. It is shown that this procedure yields a type 1 error for the entire experiment very close to the risk attached to individual hypothesis tests. The advantages in this decision procedure are discussed relative to the usual analysis of variance techniques. Industrial examples are presented utilizing this procedure. These examples indicate the practical advantage of the procedure.

The absolute rankings are plotted on special absolute normal probability paper. From this plotting an estimate of the population σ is obtained from the first $n-p$ rankings. It is shown that this linear estimate has high efficiency and low bias compared to s, the nonlinear, unbiased estimate.

The Analysis of Covariance for Incomplete Block Designs. MARVIN ZELEN.

The analysis of covariance for the general case of p concomitant variates is outlined with special reference to incomplete block designs. Both the intra- and inter-block analyses are considered. An appropriate analysis of covariance is also given when the adjustment for the treatment response depends on the differential block responses. Examples of application are included.

BOOK REVIEWS

Statistical Methods and Scientific Inference. *Sir Ronald A. Fisher.* New York: Hafner Publishing Co., 1956. Pp. viii, 175. $3.00. See review article on pages 322–30.

Nonparametric Statistics for the Behavioral Sciences. *Sidney Siegel.* New York: McGraw-Hill Book Co., 1956. Pp. xviii, 312. $6.50. See review article on pages 331–44.

Nonparametric Methods in Statistics. *D. A. S. Fraser.* New York: John Wiley & Sons, Inc., 1957. Pp. x, 299, $8.50.

RONALD PYKE, *Stanford University*

THE author states in the preface that this book "is concerned with the treatment of standard statistical problems when the familiar assumption of normality is replaced by general assumptions concerning the distribution form." The book embraces two distinct topics and should be separated into two parts, the first, of two chapters and 119 pages, devoted to "a survey of the general techniques of estimation and hypothesis testing," the second, of five chapters and 172 pages, to "an attempt to collect and unify diverse developments" of nonparametric statistics.

This reviewer believes that the book is of sufficient merit to demand a place on every mathematical statistician's bookshelf. It is clearly presented and well indexed as a reference book (especially the first part), while at the same time containing sufficient examples, motivation, and problems for a textbook (especially the second part). On the other hand, the book does *not* reduce the need for a more extensive and detailed coverage of the theories of estimation and hypothesis testing. (It is regrettable that such a text did not precede this book. The mimeographed lectures of E. L. Lehmann[1] on these topics, although serving well as a text and as an acknowledged guide for this author, could not in their present form be appropriately used as a reference book.) Neither does it cover comprehensively the field of nonparametric statistics. For example, no mention is made of the nonparametric topics associated with such names as T. W. Anderson, Z. W. Birnbaum, Cramér, Darling, Kolmogorov, Smirnov, and von Mises. Consequently, the title is misleading. The χ^2-statistic, although mentioned, is not even described. A reference to the extensive bibliography of nonparametric statistics and related topics by I. R. Savage (*JASA*, Vol. 48, (1953), 844–906) should be made in any textbook on this subject.

Chapter 1, entitled "The Sample Space," contains subsections labelled Measurable Space, Measures and Probability Measures, Expectation and Conditional Probabilities, Sufficient Statistics, Completeness, Two Examples of Completeness (the order statistic with respect to the absolutely continuous and the discrete distributions on R_1). In the preface, we are told "A knowledge of measure theory is not necessary, the essential ideas of measure being introduced with a statistical interpretation." Thus in 14 pages the reader is made familiar with measurable spaces and functions, measure, completing a measure, 1–1 correspondence between probability measure and distribution functions (of total variation 1 and suitably oriented), product measure, Lebesgue integration, absolute continuity, the Radon-Nikodym theorem, and conditional probability. That this is a heavy load for a first year graduate, necessitating a careful and extended exposition by the instructor, is clear. However, the preface declares that "The first two chapters are used at the University of Toronto for an *undergraduate* course surveying recent small sampling methods" (emphasis added). It should be pointed out that a student entering a (central) Canadian university has had five years of high school and is eligible to enter as a sophomore in

[1] A.S.U.C. Book Store, Berkeley, California, 1949.

most American universities. Thus a fourth year undergraduate at the University of Toronto is comparable to a first year graduate in the U.S.A.

It is gratifying to note that the Radon-Nikodym theorem is employed very frequently throughout the text. However, it is disturbing that it should have been stated incorrectly. The measure μ must be σ-finite, otherwise counter-examples are readily constructed (see e.g. Halmos, *Measure Theory*, D. Van Nostrand, 1950). Also, the set function defined equal to infinity everywhere (even on the null set) is a measure, according to this author's definition, thus making the theorem ambiguous.

The definition of strong completeness, as given on p. 26, is erroneous, since by this definition strong completeness is equivalent to completeness. (This error was brought to the reviewer's attention by Charles M. Stein.) A more meaningful definition would begin: "The measures $\{P_\eta^T | \eta \epsilon H\}$ are strongly complete with respect to a measure m on H if for any subset H^* of H . . .". Moreover, for the proof of Theorem 6.3 to be valid, the measure m which should enter into the hypothesis of the theorem, must be σ-finite. When m is Lebesque measure, this definition becomes the usual one as introduced by Lehmann and Scheffé.

Chapter 2, "Statistical Inference," the longest in the book (89 pages) introduces Decision Theory, and covers Estimation, Hypothesis Testing, and Confidence and Tolerance Regions. The introduction to Decision Theory, though necessarily brief, is well expressed. In Example 1.1, "The penalty for an incorrect decision is severe, say death." This novelty does not have any motivation other than to provide for later humor: "The loss is one unit, in this case one statistician, for an incorrect decision." The section on Invariant Estimation suffers more than other sections because of its brevity. However, this poor introduction to the concept of invariance is remedied somewhat by a later exposition on invariant tests. The author's use of P for power functions is confusing.

Chapter 3 is a classification of nonparametric problems and is intended to serve as a link between the general (parametric) theory of the first part and the nonparametric theory which follows.

In Chapter 4, entitled "The Estimation of Real Parameters and Tolerance Regions," U-statistics and tolerance regions are studied. The section on tolerance regions is well presented, although the intrinsic geometrical difficulties cannot be avoided.

Chapter 5, "The Theory of Hypothesis Testing," is concerned entirely with tests based on an order statistic. In particular, the uniformly most powerful tests of the location problem for absolutely continuous distributions are derived. Rank tests and the likelihood ratio method are also studied. Concerning the latter, the author states, "In the special case where the outcome is a sample of n from a distribution over a component space, the likelihood ratio test has been shown to have asymptotically good properties as $n \to \infty$." It would have been advantageous to state such properties or at least to give references.

Chapter 6, entitled "Limiting Distributions," is a compilation of limit theorems for dependent variables of the type considered by Hoeffding, Ghosh, Dwass, Wald and Wolfowitz, and Robbins. Prior to this, a summary of the pertinent definitions and theorems of probability theory are quoted, page references being given to other texts for the proofs. ("Independent" may be removed from the statement of Theorem 2.6.) The reference to Feller's text for the proof of Kolmogorov's strong law of large numbers (Theorem 3.2) is not suitable since Feller concerns himself only with discrete probability measures. Loève's *Probability Theory* (D. Van Nostrand Co., 1955), p. 239, would be a more appropriate reference. This chapter is well-presented, although its notation is heavy. Theorems 4.4, 5.1, 5.5 and 6.4, of which only one is proven, require four pages of notation before they may be quoted. This, however, is more a criticism of the subject matter than of the author's presentation.

Finally, Chapter 7, "Large Sample Properties of Tests," gives an excellent study of the relative efficiency of tests as proposed by Pitman and generalized by Noether. Theorem 3.1 which is given without reference should be attributed to Pitman, even more so than a consequence of it, namely Theorem 3.3, which is so attributed.

Some miscellaneous points: At the end of each chapter is a large set of problems, two of the chapters having over 40 problems each. The problems are well motivated and instructive. (Problem 9, p. 119, is incorrect.) In the later chapters a few of the problems involve extensive computations. The appearance of the book, both with respect to type faces and layout, is conducive to easy reading. There are numerous printer's errors, 49 having been listed by this reviewer without really hunting. Discouraging words, such as "obvious" and "straightforward" fortunately appear very rarely, but on p. 19, the author declares, "It is seen directly from the definition of a sufficient statistic that the following theorem is obvious." The references and the bibliography at the conclusion of each chapter are sufficiently detailed, but do not seem to be in any consistent order. The majority of theorems are labeled according to their authors, the system which this reviewer prefers, although in a few instances this may give rise to disagreement.

In conclusion, the book is a worthy addition to existing texts on theoretical statistics at the graduate level. It is the first such book on nonparametric statistics and as such is of great value.

An Introduction to Statistical Analysis. Second Edition. *Wilfred J. Dixon and Frank J. Massey.* New York: McGraw-Hill Book Company, Inc., 1956. Pp. xiii, 488. $6.00.

WILLIAM G. COCHRAN, *Johns Hopkins University*

As many readers will know, this book has proved a popular introductory text for a general one-year course at a mathematical level involving only a knowledge of algebra. The second edition follows the same outline as the first. A new chapter on probability presents the additive and multiplicative laws, permutations and combinations, and leads up to the hypergeometric, binomial and Poisson distributions. The chapter on statistical inference has been enlarged to include more study of errors of the second kind. Chapter 10 (analysis of variance) is rewritten; new topics are a series of illustrations of the types of problem that can be handled by the analysis of variance, more extensive discussion of *t*-tests and an introduction to the 2×2 factorial. Numerous smaller sections have been inserted to describe more recently-developed techniques, thus maintaining the "up-to-date" character of the first edition.

A feature of the first edition was the unusually large assembly of statistical tables. These now occupy 114 pages, as against 72 in the first edition. Among the newcomers are the power functions of the *t*- and *F*-tests, various quick tests based on ranges and ranks, binomial tables, reciprocals and square roots.

As its title implies, the emphasis in this book is on statistical techniques. Its strong points are (i) clear exposition of the techniques, with some account of assumptions required and of limitations, and (ii) inclusion both of the older orthodox procedures and of newer methods seldom found in elementary texts. For a well-rounded introductory course in statistics, its chief deficiencies are a scarcity of illustrations of the techniques by applying them to real data, and only a minimal discussion of the initial stages of a statistical investigation—the definition of the problem and the planning and collection of the data. In these respects the book may be contrasted with Snedecor's *Statistical Methods*, which relies much more heavily on illustrations of the uses of techniques as a teaching device, and with Wallis and Roberts' *Statistics: A New Approach*, in which the emphasis is on the planning and execution of investigations, with a more subsidiary role assigned to techniques of analysis as such.

Statistical Methods in Research and Production: With Special Reference to the Chemical Industry. Third Edition. *Owen L. Davies*, Editor. Edinburgh: Oliver and Boyd, 1957. Pp. x, 396. 45*s*.

THE revisions in this work, the first edition of which was reviewed by D. B. DeLury and A. E. R. Westman in this *Journal*, Vol. 43 (1948), pp. 318–20, are described on the jacket as follows:

> The revision for this Third Edition has been very extensive. New topics include the economics of testing and experimentation; sequential sampling; the estimation of variance components and their confidence limits; and the analysis of covariance. An extra chapter on relationships between variables has been added to permit a fuller treatment of multiple curvilinear relationships, with particular reference to difficulties of interpretation. In the two chapters on relations between variables, a clear distinction is made between regressions and functional relationships. Throughout the book the emphasis is on the use of confidence limits in preference to tests of significance, experience having shown the greater practical utility of the former.
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> W. A. W.

Curso de Estadística, Volumen II. *P. Enrique Chacón*, S. I. Volumen IV de Publicaciones de la Universidad de Deusto. Bilbao, España: Editorial El Mensajero del Corazón de de Jesús, 1956. Pp. xvi, 526. 200 ptas.

JORGE ARIAS B., *Universidad de San Carlos de Guatemala*

THIS volume is the second part of the work reviewed in the December 1956 issue of this *Journal*. As was pointed out at that time, the original intention of the author was to publish two volumes. However, in the introduction to this second volume, he confesses that he later realized that three volumes would be required for all of the material he desired to include. Therefore, this second volume contains less material than was mentioned in the previous review.

Volume II contains Chapters 24 to 48 which include an introduction to sampling theory, confidence limits, hypothesis tests, and analysis of variance. The technical aspects of sampling are covered in the third volume which has also been published.

Chapter 24 (4 pages) contains generalities on inductive methods, and the next chapter (9 pages) defines random sampling and discusses the construction and use of tables of random numbers. Chapter 26 contains, as an illustration, an introduction to decision functions and statistical games. The theory of estimation and the study of confidence limits and tolerance limits are the topics covered in the three following chapters, which contain 33 pages.

Chapters 30–34 (187 pages) refer to the theory of estimation and its applications to the estimation of the more currently used statistics, the fixing of confidence limits and testing of such statistics. It includes a discussion of problems on normal and Poisson distributions, and also on distributions that follow unknown laws. Tests of normality and other similar tests are described in the 10 pages that constitute Chapter 35.

The comparison of averages, using parametric and nonparametric methods, and comparison of variances are studied in the next three chapters (60 pages). The comparison of two or more normal populations is the object of Chapters 40 and 41 (34 pages). These are followed by the analysis of variance, for simple and multiple classifications, touching the case of nonnormal populations. This matter is covered in three chapters with 65 pages.

The application of theory of estimation to regression problems, and to joint distributions of two or more random variables is discussed in Chapters 44–47 (47 pages). Finally, Chapter 48 reviews the principal test of randomness.

A collection of 29 statistical tables relating to topics covered in this volume, and three indexes, one of subjects, one of symbols, and one of authors, complete this part of the work.

In general, the same comments made in the review of the first volume can be made for this second section. To the knowledge of the reviewer, Chacón's book constitutes the most extensive work on statistical methods written in Spanish. The printing of the mathematical formulas is clear and careful. The bibliography is up to date. The exposition is clear, and the mathematical developments are well divided into their various stages, but it must be pointed out again that this book is not suitable for students without a solid mathematical background. It is regrettable, however, that this volume contains only a limited number of problems, most of them included in the text. There are few chapters that contain a specific section on problems, and in these cases answers are not given. The problems used for illustration relate in great part to quality control topics. Other problems do not explain any relation to real situations, thus losing part of their usefulness, especially for illustrating interpretation of the final results of the solution of a problem.

Tables for Use with Binomial Samples. *Donald Mainland, Lee Herrera, and Marion I. Sutcliffe.* Department of Medical Statistics, New York University College of Medicine, 1956. Pp. xix, 83. $2.00. (Obtainable from the authors, New York University College of Medicine, 550 First Avenue, New York 16.)

D. J. FINNEY, *University of Aberdeen*

THIS volume extends and makes more readily available tables that the authors have previously published in medical journals. For rapid significance testing and associated problems connected with binomial samples and 2×2 contingency tables, it will be found exceedingly useful. These are not primary tables of mathematical and statistical functions comparable with those produced by the National Bureau of Standards; nevertheless, their numerical accuracy should be adequate for the vast majority of questions on the precision of estimation from one, or the comparison of two, binomial samples encountered by the practicing statistician. For instance, if tests of significance were needed on a long series of 2×2 tables, the tables would give most of the answers by direct reference, leaving a necessity for only a small number of more laborious detailed calculations.

Tables I and II enable tests of significance (at the 5% and 1% levels respectively) to be made rapidly for any 2×2 contingency table in which two samples of size $N = 4(1)20(10)100(50)200(100)500$ are compared. The condensed arrangement is ingenious. Under any value of N appears a series of pairs of numbers in the form n_1/n_2, meaning that if one sample has n_1 "successes" the difference is statistically significant if and only if the second sample has at least n_2 "successes." For example, part of the tabulation under $N = 150$ in Table I reads " . . . 15/28 16/30 19/33 20/35 . . . "; therefore, the differences between 15 and 28 out of 150, 16 and 30 out of 150, 19 and 33 out of 150, and so on are just significant at the 5% level, and by convention the gap between $n_1 = 16$ and $n_1 = 19$ is to be filled with "17/31 18/32." The basis of the test is of course the Fisher-Yates exact probability formula. When n_1 is smaller than $N/2$, the pairs of critical values change very slowly with N, so that interpolation can easily be used for values of N not directly tabulated; moreover, the tables often suffice even when two samples of unequal size are to be compared.

Table III provides the sum of the Fisher-Yates probabilities (to 4 decimals) from one tail for all possible outcomes of a 2×2 contingency table having two equal samples of $N \leq 20$. Table IV gives similar information for unequal samples of 20 or less, but is restricted to those combinations of frequencies that fall beyond but

closest to the 2.5% and 0.5% significance levels (one tail). Thus Table IV gives much the same information as tables published by the reviewer and by Latscha, except that they also gave 5% and 1% levels but quoted probabilities to 3 decimals only.*

Tables V to VIII provide what are perhaps somewhat loosely described as "confidence limits" for the results of single binomial samples. These have been calculated from Fisher and Yates's table (originally due to Stevens) of "Limits of the Expectation" for the binomial distribution. They occupy much more space than the very compact original table, but save the user much irksome interpolation. Although the authors have included one digit more than is strictly justified by the original table, tests they have made show that in general interpolation is thereby improved. Tables V and VII give 99%, 95%, and 80% limits for samples of size 2(1)30(5)50(10)120 and occasional additional values, with some higher values up to 1000, and for 1(1)14 "successes." Tables VI and VIII are intended primarily for larger samples, ranging up to 100,000 with a percentage of "successes" equal to 50(1)1(0.1)0.1. A little unfortunately, the abbreviation "M" is used for "thousands," although in recent years this has become increasingly accepted, at least in Britain, as representing "million."

Table IX was directly computed to give the corresponding upper limit for a binomial sample of size 1(1)30(5)100(10)200 and values to 1000 in which no "successes" are observed.

Table X is intended to assist in the planning of comparisons of two binomial samples. It shows the frequency with which the evidence of two equal samples will be judged significant at the 5% level (i.e. by reference to Table I) when the true probabilities of "successes" in the two populations have various specified values, the information being given for samples sizes from 5 to 100.

An introduction describes the methods of construction and use of these tables, including methods of interpolation.

Symposium on Monte Carlo Methods. *Herbert A. Meyer*, Editor. New York: John Wiley and Sons, Inc., 1956. Pp. xvi, 382. $7.50. Planographed.

JOSEPH M. CAMERON, *National Bureau of Standards*

THIS volume consists of 20 papers presented at a symposium on Monte Carlo methods held at the University of Florida in March 1954. The papers fall into three main categories.

(a) *Papers on random number generation.* Olga Taussky and John Todd ("Generation of pseudo random numbers") give a survey of the different methods for generation of random number sequences on electronic computers. One concludes from this paper that sequences generated by a multiplicative scheme of the form $r_n = r_0 r_{n-1} \pmod{b^k}$ where b is the base of the machine are to be preferred over sequences using the mid-square method (which have shorter periods), and over an additive scheme based on a Fibonacci series (which does not pass some of the usual tests of randomness). A paper by James W. Butler ("Machine Sampling from Given Probability Distributions"), gives an account, with examples, of methods for producing random numbers from other distributions given random numbers from the rectangular distribution. In the other two papers in this category one, by E. J. Lytle ("A Description of the Generation and Testing of a Set of Random Normal Deviates") discusses the successful attempt to generate random normal deviates from the RAND table of random digits. The other by N. Metropolis ("Phase Shifts—Middle Squares—

* Finney, D. J., "The Fisher-Yates test of significance in 2×2 contingency tables," *Biometrika*, 35, (1948), 145–56.
Latscha, R., "Tests of significance in a 2×2 contingency table," *Biometrika*, 40 (1953), 74–86.

Wave Equation") gives properties of the mid-square method of generating random numbers.

(b) *Theoretical papers on Monte Carlo methods.* For the statistician the paper by H. F. Trotter and J. W. Tukey ("Conditional Monte Carlo for Normal Samples") and the paper by Herman Kahn ("Use of Different Monte Carlo Sampling Techniques") are probably of most general interest. The four articles, "A General Theory of Stochastic Estimates of the Neumann Series for the Solution of Certain Fredholm Integral Equations" by G. E. Albert, "Application of Multiple Stage Sampling Procedures to Monte Carlo Problems" by A. W. Marshall, "Neighbor Sets for Random Walks and Difference Equations" by T. S. Motzkin and "A Monte Carlo Technique for Obtaining Tests and Confidence Intervals for Insurance Mortality Rates" by J. E. Walsh are directed more towards the problem of use of variance reducing techniques in the case of an analytic model of a physical process.

The paper by Herman Kahn is a very readable summary of the various methods of variance reduction for the most part developed by the author himself. These methods seem to be less ideally suited to the use of Monte Carlo methods on problems in mathematical statistics than the "conditional Monte Carlo" of Trotter and Tukey and the method of antithetic variates of Hammersley (J. M. Hammersley and K. W. Morton, *Proc. Cambridge Phil. Soc.*, 52 (1956), 449–75). Nevertheless, because of the phenomenal speed of current computers one may still obtain, at reasonable cost, sufficient accuracy with a less efficient method, particularly if the saving in programming or analysis is substantial.

The basis for the "conditional" Monte Carlo method is presented in the article by Trotter and Tukey. An excellent example of the use of the technique is given in the paper "Monte Carlo Techniques in a Complex Problem about Normal Samples" by H. J. Arnold, B. D. Bucher, H. F. Trotter and J. W. Tukey, where the problem is to determine J_0 for which (in samples of 4)

Prob (largest gap $\geq G_0$ *or* the sum of two adjacent groups $\geq H_0$ *or* the range $\geq J_0$) = .05.

Savings of 500 to 1 and 5000 to 1 were achieved over the "unconditional" approach.

The paper by J. H. Curtiss ("A Theoretical Comparison of the Efficiencies of Two Classical Methods and a Monte Carlo Method for Computing One Component of the Solution of a Set of Linear Algebraic Equations") compares the amount of work involved in the Monte Carlo approach to solving, to a given accuracy, a system of linear equations with that using two classical methods.

(c) *Applied papers.* The most important applications of Monte Carlo techniques have been in the field of nuclear physics, examples of which are given in "An Application of the Monte Carlo Method to a Problem in Gamma Ray Diffusion" by Martin J. Berger, "Monte Carlo Computations" by Nancy M. Dismuke, and "Stochastic Calculations of Gamma Ray Diffusion" by L. A. Beach and R. B. Theus. These articles will probably be of interest only to those statisticians closely connected with radiation and diffusion problems.

There is an extensive annotated bibliography of 87 pages which would be invaluable aid to those just beginning the subject. For the statistician unfamiliar with Monte Carlo methods the excellent introductory note by A. W. Marshall, the papers by Kahn and by Trotter and Tukey along with the bibliography would constitute a good introductory course in the subject.

The book is valuable as a reference, and indispensable to those involved in Monte Carlo applications.

Electronics in Management. *Lowell H. Hattery and George P. Bush*, Editors. Washington, D. C.: University Press, 1956. Pp. xii, 207. $6.00.

H. BURKE HORTON, *Office of Defense Mobilization*

THIS collection of articles in the field of computer applications to management-type problems is well integrated and very informative. The coherence of the selections results from the fact that they were prepared as part of a special lecture series (First Institute on Electronics in Management, American University) rather than as a collection of specialized papers of primary interest to the various authors.

The papers are grouped as follows: I. Management Impact; II. Equipment; III. Case Examples; IV. Suggestions for Management.

Part I contains some far-sighted remarks about the company-wide or plant-wide teamwork and the advance preparation necessary to realize full benefits from a large electronic computer. Part II covers the principles of computer operation and the state of the art in this country and in Europe. An unfortunate delay in the visas of Russian participants in a computer conference at Darmstadt prevented a careful appraisal of the state of Russian technology in this field; however, the impressions formed by the brief contacts between their specialists and other Europeans indicated that they had reached about the same stage as the rest of the continent. Subsequent information received concerning Russian work has confirmed this general impression.

European computers are tracing the path of the United States a few years ago. The early machines are "introverts," capable of fairly satisfactory speeds internally, but with unsatisfactory input-output speeds for communication with the outside world. As a consequence, their early applications, like those of the United States, have been primarily in the fields of mathematical and scientific problems.

Part III contains four excellent case examples of computer applications to problems of the Bureau of the Census, the Air Materiel Command, the Naval Aviation Supply Office, and the Treasury Department. A careful reading of these case studies should enable management personnel to avoid costly delays and errors in the installation of new systems. The authors agree that the emphasis should be on complete systems analysis rather than simply the application of the new hardware to existing procedures.

Part IV contains articles by eight authors from industry, government, and university campuses. They are filled with worthwhile "Suggestions for Management." The suggestions range from systems management to efficient stocking of spare parts. These suggestions, following the case examples, will give prospective users ideas on how to stay on top of the varied problems involved in computer applications.

Taken as a whole, the authors of the papers contained in this book do a good job of giving newcomers the "feel" of the relationship of electronic computers to management problems. American University is to be commended for gathering this talent for a lecture series and for publishing the collection of papers. The authors do not attempt to pick the best computer for a given job—that you still have to do for yourself.

Random Processes in Automatic Control. *J. Halcombe Laning, Jr. and Richard H. Battin.* New York: McGraw-Hill Book Co., 1956. Pp. ix, 431. $10.00.

EMANUEL PARZEN, *Stanford University*

THE years since the Second World War have seen the development among electrical engineers of a field of research which may be called statistical communication theory. An increasing number of electronic devices, such as radar, sonar, and

weapons fire-control systems, are essentially analogue computers concerned with the transmission, extraction, or detection of signals in the presence of noise. Statistical communication theory is the theory of the design of such devices. On the one hand, it is used to analyze the behavior of devices (or systems) which have been proposed for communication or control purposes, and on the other hand it seeks to construct systems which achieve a given aim in an "optimum" manner. Thus, research in statistical communication theory has been concerned equally with finding "good" systems and with finding "best" systems for the transmission or detection of signals in the presence of noise.

Many problems of statistical communication theory may be stated as problems of statistical inference on continuous-parameter stochastic processes, in the following way. An observer, listening at a receiver for a time interval of length T, receives a message, represented by $y(t)$, defined for $0 \leq t \leq T$, which is assumed to be a random function (of time) possessing finite second moments. It is assumed that $y(t)$ is the sum of a random function $N(t)$, the noise, and a function $S(t)$, the signal. The noise $N(t)$ is assumed to have mean 0, for all t, and a known covariance function $\mathrm{E}[N(t_1)N(t_2)]$. Often $N(t)$ is assumed to be normally distributed. About the signal $S(t)$ there are a large variety of assumptions which may be made; $S(t)$ may be a noisy signal, by which is meant that it is similar to $N(t)$, except for a different covariance function, or $S(t)$ may be a sine wave, $A \cos(wt+\theta)$, where the phase θ is random with known probability distribution and A and w are constants which are to be estimated. More generally, $S(t)=g(t; V_1, \cdots, V_m; \alpha_1, \cdots, \alpha_k)$ where $V_1, \cdots, V_m$ are random variables and $\alpha_1, \cdots, \alpha_k$ are parameters.

In general, there is a known family of possible signals which may be the received signal $S(t)$. One's interest may be in transmitting one of the family of signals in the presence of the noise $N(t)$; then the problem of the observer may be stated as a problem in statistical estimation, namely, given the observation $y(t)$, $0 \leq t \leq T$, to estimate $S(t)$, $0 \leq t \leq T$, or some functional of $S(t)$. On the other hand, one's interest may be in detecting whether or not one of the possible signals was received, in which case the communication problem may be stated as one of statistical hypothesis testing, namely, given the observation $y(t)$, $0 \leq t \leq T$, to test the hypothesis that noise alone was received against the alternative hypothesis that signal plus noise was received.

The book by Laning and Battin is an introduction to statistical communication theory in the language of electrical engineers. It does not point out the connection between statistical communication theory and the theory of statistical inference on stochastic processes. Its bibliography contains no reference to the statistical literature (although a few basic probability texts are listed).

The book is self-contained mathematically. A brief introduction to probability theory is given in Chap. 2 (81 pages), while Chap. 3 (47 pages) discusses the description of a random process. The authors adopt a modern point of view. They emphasize that, abstractly, a random process is a sample space of functions on which a probability distribution has been defined. The discussion of random processes is satisfactory, but not elegant. The notion of an ergodic process is introduced, but not in a precise enough form so that conditions could be stated for a stochastic process to be ergodic. The notion of the power-spectral density of a stationary time series is introduced in the manner which is customary among engineers, and certain errors are consequently perpetuated; no account is taken of recent relevant work by statisticians on statistical spectral analysis of stationary time series. Chap. 4 (30 pages) states the main properties of the shot effect and Gaussian random processes. The discussion is not entirely rigorous, especially since the Poisson stochastic process is not treated explicitly.

Chaps. 5 and 6 (92 pages) are concerned with the mathematical description of filters, and of filtered stochastic processes. Chap. 7 (22 pages) discusses in engineering language Wiener's theory of prediction and filtering. Chap. 8 (68 pages) turns to the problems of statistical inference sketched at the beginning of this review, although the material is not labelled as such. Much of Chap. 8 is original with the authors.

The book contains certain valuable material. In particular, it contains a large amount of *practical* material on the solution of various integral equations which arise in the theory of statistical inference on stochastic processes. It also contains a number of interesting examples. On the other hand, the theoretical material in the book is presented rather inelegantly and nonrigorously, and in language which obscures the statistical issues involved. It should be pointed out, however, that the book was conceived in 1951. It is developments of only the last few years that the reviewer has in mind when he expresses the opinion that the material in this book is now capable of a rigorous and elegant formulation on the basis of the notion of the likelihood functional of a stochastic process.

International Journal of Abstracts on Statistical Methods in Industry. Published by the International Statistical Institute, under the auspices of its Committee on Statistics in Industry and Technology. 2 Oostduinlaan, The Hague, Netherlands. Annual subscription (three issues) $2.50; single copies $1.00.

THIS bibliographical journal, now in its fourth year, publishes about 350 abstracts per year, classified under five headings: Process Control, Sampling and Acceptance Inspection, Research and Development, Administrative Applications and Operations Research, and Management Applications. Papers and books "published in all parts of the world" are covered by brief abstracts nearly always in English but occasionally in French which are intended to enable "the reader . . . to judge for himself whether or not the original article or book would be of interest to him." The 1956 volume represented 96 journals, 23 books and brochures, and 37 abstractors (including 2 organizations). An annual index classifies the abstracts by statistical subject, by technological subject, by administrative subject, and by author. There seems to be no coverage of Russian literature. The abstracts are printed on thin cardboard, on one side only, ruled for cutting into cards 2¼ by 4¼ inches; the printing is photographic reproduction of typewritten copy.

W. A. W.

Revue de Recherche Opérationnelle. *E. Ventura*, Editor. Published by the Société Française de Recherche Opérationnelle, Paris. 500 F.

THIS small French magazine, which appeared as a quarterly in the last quarter of 1956, is the official publication of the Société Française de Recherche Opérationnelle, the first of its kind in French. It contains three articles by specialists of France, information on the Société and its members, book reviews, and a page entitled "Problèmes plaisans et délectables."

D. D. F.

Business Forecasting in Practice: Principles and Cases. *Adolph G. Abramson and Russell H. Mack*, Editors. New York: John Wiley and Sons, Inc., 1956. Pp. xiii, 275. $6.50.

S. JAY LEVY, *Industry Forecast*

BUSINESS forecasting has a fascination beyond tales of how brilliant predictions led to financial coups. Business forecasting is the test we have of the quality of economic knowledge.

The editors of *Business Forecasting in Practice*, aside from expecting it to aid in the preparation of forecasts, hope that it "will serve as a useful supplement to existing

literature on Business Cycles." The book is enthusiastic about forecasting. It attempts to present forecasts as the satisfactory products of business cycle theory. It succeeds, indeed inadvertently, in raising doubts about the importance of recognized business cycle theory. It presupposes that forecasters apply economic theories to achieve their practical ends. It reveals that they hardly ever rely on formal theory. Fortunately for academic economists, most readers of this book will be not philosophers but men trying to make headway in the worlds of business, finance, and perhaps political administration.

Business Forecasting in Practice begins with an efficient survey of "Business Cycle Theories" by one of its editors, Russell H. Mack, Professor of Economics at Temple University. Chapter II, "Techniques of Forecasting" by co-editor Adolph G. Abramson, economist for SKF industries, suggests the snubbing of theoreticians that is to come. Abramson presents a "logical and reasonable way to approach the forecasting problem: to first establish the causes of the movements and then develop ways of measuring or evaluating these causal forces.

"This process, however, involves numerous choices or practical operating problems. *Which explanation is valid at the moment?*" (Reviewer's italics)

Against the broad background provided by the editors, six forecasters employed by private business and financial enterprises courageously present examples of their work. They follow a common outline: (a) an actual short-term forecast, (b) the causal explanation or theory of the business cycle which was used, (c) techniques of forecasting used, (d) data used, (e) how the general business forecast was converted into a company or industry forecast, (f) uses made of the forecast.

Joseph B. Hubbard of Union Service Corporation, in following (b) of the outline, says that he "relies basically upon observation and description of the data and recognition of similarities and dissimilarities between given situations in the present and the past, rather than upon assignment of causal connections among the items in question." In speaking of "the anxiety often expressed that a forecast will be upset by an outside force, such as represented by war or a large error in the statistical data," Hubbard gives an important, constructive argument. " . . . Occurrences of this nature (wars) are not secret or hidden. . . . They are in fact often anticipated well in advance. Bad statistics, similarly, in general become well advertised. . . . "

A vice president of Federated Department Stores was forecasting in 1953 by exploring the economy piece-meal, rather than on the basis of the theories digested by Mack. Myron S. Silbert considers the influence of personal and business taxes, the prospect of automobile demand, the outlook for farm income, potential foreign factors, eleven other items he lists, and many more. He weighs the favorable factors against the unfavorable ones to chart the future course of the economy. Silbert's 1953 predictions were applied not only to general policy but to the supply and demand situations of ten specific categories of commodities.

William W. Tongue, who presents a particularly interesting discussion of how the Jewel Tea Company utilizes the forecast, confesses, "For many years I have found it difficult to reach satisfactory conclusions about the cause or causes of business fluctuations, in the sense of a chain of circumstances which would hold for all time or in all circumstances.

While impressing his readers with the rather direct benefits which the investment counselling firm of Stein Roe & Farnham derives through forecasting, partner Kenneth D. Ross exhibits the talent of nicely stating what most of his fellow forecasters seem to observe: "The attempts to cast business in a cyclical mold have actually tended to hamper the forecaster."

Donald J. Watson's forecast in 1953 "was not cast in the framework of any particular business cycle theory. In fact, the economic situation in mid-1953 did not appear to fit into the pattern of any formal theory since the forces propelling the boom were outside the scope of most of the recognized theories." Watson's description of how General Electric used his forecast constitutes an effective glimpse into the much discussed and admired organization of this corporation.

Wilson Wright, the final contributor, tartly dissents; he accepts a cycle theory. He points out the ignorance of ordinary persons attempting to appraise the business outlook. "The major business cycles seem to be the consequences of war and the way in which national financial systems are used by persons engaged in buying or selling commodities. Fluctuations of this order are so large, and are experienced over such a long period of time, that they ordinarily are ignored by persons attempting to appraise the outlook for business activity."

Wright, economist for Proctor and Gamble, refuses to adhere to a limited listing of statistical series and opinion surveys for the data of his forecast. He holds that "the task of appraising the economic outlook consists of determining what people performing different economic functions probably will do . . . there is little information concerning such matters which is not useful." However, debatable Wright's theories and methods, this statement of the problem of forecasting emerges as among the clearest of the book. It suggests that a cycle theory (even Wright's) may so concern the forecaster with certain types of data that he will lose sight of the decision-making processes of persons who are, after all, the creators of the business climate.

The actual forecast of each of the six writers concerns the business decline of either 1948–49 or 1953–54, its magnitude, and duration. Of the greatest interest to the forecaster who has this splendid opportunity to observe the work of others is their meaning of the term "forecast" and the techniques they employ.

The 1948–49 recession began in December 1948 and reached bottom in October 1949; the 1953–54 decline was underway in August 1953 and had run its downward course by August 1954, according to the National Bureau of Economic Research. Judging by the forecasts presented in this book, "forecast" has a surprising definition.

Case A: September 1948: "The business boom, although it has taken on increasingly the characteristics of a matured boom, will probably persist into 1949."

January 1949: "An increasing number of industrial readjustments are in prospect and some decline in general business activity is to be expected. . . . "

Case B: April 1953: " . . . we make the estimate that after June 1953 the total business picture will turn downward but that the decrease will be moderate."

Case C: July 1949: " . . . the bottom of the recession may come earlier than anticipated, perhaps in July or August of this year."

Case D: June 1953: " . . . activity has reached its peak and will turn down before long."

Case E: July 1953: "A general business decline is expected to begin during this quarter. . . . "

Case F: (an undated forecast of the 1948–49 recession).

The typical forecast, if we are to judge by the above examples, anticipates a change in general business conditions about two months ahead. It is made when these changes are already affecting a substantial portion of business activity. Since probably none of the forecasters who contributed to the book was coerced into submitting his records, it can be concluded that a *good* prediction, in the eyes of its author, is one which is alert to changes *currently* taking place and not necessarily one that anticipates a change even as little ahead as one or two quarters.

The forecasters on the occasions recounted served their organizations well by stating within the limits of reasonable accuracy the degree of the decline. When the decline was beginning, at least, they had a fair idea of its duration.

These seven responsible forecasters, Abramson and the six who have offered their case histories, represent a cross section of outstanding business and financial organizations. They virtually blanket the subject of forecasting techniques. Their approaches, however, generally lack the boldness which conclusions about the unknown future seemingly require. By relying on experience and history as their chief guides and slighting or even ignoring causal theory, the forecasters are in the position of scanning the horizon and not attempting to ascertain what lies beyond it.

While this difficulty may arise from their failure to find a theory which has consistent validity, it is aggravated by the forecasters' emphasis on economic activities which reflect business decisions, rather than on those that influence decisions. Predictions are generally concerned with production, gross national product, income. Regardless of how high or low these activities may be, a crucial question remains. Will business be profitable in the next quarter or next half? The answer to this question will indicate whether managements will change or intensify policies that affect various pertinent factors, from capital expenditures to instalment credit terms.

Such an approach is suggested at the beginning of the book. Mack quotes Wesley C. Mitchell: " . . . we must recognize that profit making is the central process among the congeries that constitute the activities of a business economy. Weather conditions count insofar as they affect profits, so do emotional aberrations, so do the production and consumption of consumers' goods, and so do a thousand other factors. On the other hand, the prospects of making profits react upon all these other processes, insofar as they are affected by human behavior."

Perhaps monetary, psychological, investment, innovation and other business cycle theories are ways of analyzing the single question of profitability. The business executive who reads this book may, because of his concern with profits, synthesize the causal approaches to forecasting into the question: Will the business psychology, as I represent it, result during the next half in a favorable reaction on my part to the profit-making possibilities of using cash and credit to invest, innovate, speculate?

Production Forecasting, Planning, and Control, Second Edition. *E. H. MacNiece.* New York: John Wiley and Sons, Inc., 1957. Pp. viii, 374. $5.90.

L. G. Mitten, *Ohio State University*

The success of the first (1951) edition of this book is ample indication of the author's ability to condense his very considerable practical knowledge and experience into a highly readable and practical volume. The compass of the book is broad, covering the many facets of production management—from sales forecasting, organization, and factory planning, through procurement, loading, scheduling, etc., to relations with accounting and quality control. Consideration is also given to such topics as level production and stable employment, rules of personal conduct in an industrial organization, and the general approach to the solution of forecasting, planning, and control problems. The problems and cases in each chapter are supplemented by a rather extensive two-chapter practical problem and its solution toward the end of the book.

Several significant additions have been made in the second edition. A short chapter on automation and operations research gives a general description of these two fields and an appraisal of their place in the industrial scene. A chapter on specialization, standardization, and simplification of products and operations has been added. The author draws on his recent experiences as Chief of Management Education and

Training in Europe for the Foreign Operations Administration and as Special Representative in Europe for Johnson & Johnson for a final chapter on the state of production management in Europe. Also discussed in this chapter (and several paragraphs at other points in the book) are some of the special problems and techniques of production planning and control encountered in Europe.

Other chapters have been supplemented by sections on multiple machine operation, optimal size of factories, and the "85–15" concept of inventory control. A number of minor changes and additions are to be found, including a very brief description of the ratio-delay technique. A minor rearrangement of chapters has improved the continuity, and the bibliography has been expanded, as have the number of problems at the end of each chapter.

The changes in the second edition have brought it up to date without substantial revision of the original organization and text. This book should continue to serve as a good introduction to the field; however, its appeal to the more mathematically inclined reader will probably remain slight.

Problems of Capital Formation: Concepts, Measurement, and Controlling Factors. Studies in Income and Wealth, Vol. 19, National Bureau of Economic Research. Princeton: Princeton University Press, 1957. Pp. xi, 612. $7.50.

GERALD J. MATCHETT, *Illinois Institute of Technology*

THIS volume contains a collection of 15 papers delivered at the meetings of the Conference on Research in Income and Wealth held in the fall of 1953, together with the comments of participants. The first 12 papers are primarily concerned with the measurement of private capital formation while the three concluding papers attempt to explain the factors controlling the amount of private capital formation.

The first three papers of Part I are devoted to estimates of various components of capital formation and to a discussion of the underlying methods of estimation. In the first of these, David M. Blank and Louis Winnick present new estimates of gross and net private nonfarm residential capital formation. Official estimates of expenditures for new dwellings and for additions and alterations have been available for the period since 1915. The new series extends these estimates back to 1889, thus throwing new light on long term swings of gross capital formation in residential construction. The entire series on capital consumption allowances presented in this paper is based on new estimates. Statistically, the amount of depreciation is calculated as the difference between the current market value and the estimated current cost of production.

In the next paper, James P. Daly, calling attention to the difficulties of measuring inventory investment in estimating capital formation, reviews available estimates for the period since 1919. In doing so, he performs the useful service of examining and comparing estimating procedures and underlying assumptions as well as the sources of basic data. Kenneth Buckley performs similar tasks for the statistics on capital formation in Canada. Since descriptions of the official estimates that go back to 1926 have been published, this paper concentrates on the estimates that are available from 1900 to 1930.

In the next paper, Daniel H. Brill presents estimates of the forms in which various sectors of the economy have financed their additions to the stock of fixed assets. He analyzes the financing patterns of consumers, corporations, and state and local governments and emphasizes the diversity in patterns among these major groups. If there is a common thread to the experience of these groups, Brill concludes that it is the development, in the post war period, of a decrease in liquidity and an increase in fixed charges on incomes.

In the last two papers of Part I, George O. May and Edward F. Denison discuss some of the problems that arise in any measurement of the net change in capital stock. The task of estimating net capital formation is complicated enormously by the role of obsolescence, which is frequently far more significant than physical deterioration as the cause of the retirement of assets. May presents an historical sketch of changes in the accounting treatment of capital items during the last fifty years with interesting illustrations drawn from a number of industries. In various ways, he points out limitations of accounting sources in deriving statistics of capital formation.

In one of the most interesting papers on the theoretical aspects of quality change, capital consumption, and net capital formation, Denison is mainly concerned with two problems. The first is the problem of quality change: how can the constant dollar values of machine tools made in 1930 and 1953 be compared? The second is the problem of allocating capital consumption over time: if a house costs $10,000 in 1952, how much value was used up in 1953? In answer to the first question, Denison concludes that the most fruitful and practical way of measuring the real stock of capital in existence at a given time is to evaluate the production or reproduction cost of this stock at the prices prevailing in some period taken as a base. Thus the emphasis is on cost of production rather than productivity. In answer to his second question, Denison proposes to obtain net capital formation by subtracting a suitable capital consumption allowance determined by current age and life expectancy of the component. In the discussion following this paper, Simon Kuznets and Eric Schiff point out that Denison's method would yield different estimates of the effect of technological advance on capital stock, depending on whether the technological development reduces the cost of a given facility or increases its productivity. Denison recognizes this shortcoming and others as well; however, his defense of the method is generally convincing.

The contributions of Part II are primarily devoted to the estimation of capital coefficients and productive capacity. The authors are concerned with both the conceptual problem and the presentation of quantitative results and description of estimating procedures for a number of specific industries.

In the first paper in this part, Anne P. Carter gives an account of variability problems in the prediction of capital requirements. The next three papers deal with estimates of capital coefficients for specific industries: Frederick T. Moore for the mineral and metal industries, Raymond T. Bowman and Almarin Phillips for metal fabricating industries, and John E. Hodges for the petroleum industry.

In the next paper, Robert N. Grosse and Edward B. Berman deal with the problem of predicting replacement needed to maintain capacity at its exact current level. Under rather restrictive assumptions, the authors demonstrate a method for estimating replacement requirements using only capital coefficients, capacity time series, and equipment lives. Although the results depend on hypotheses open to serious question, the paper presents an interesting approach to a practical problem which is complicated both by the absence of a satisfactory theoretical framework and by the inadequacy of available statistical data. Moreover, as the authors point out, estimates of replacement requirements would be facilitated if companies followed more uniform and rational procedures in determining optimum replacement policy.

In the final paper of Part II, Bert G. Hickman presents empirical findings about the cyclical behavior of capacity and capacity utilization with particular attention devoted to the acceleration principle. The author has collected a valuable body of data showing annual estimates of capacity and production for a number of industries over several decades. The analysis of the author and that contained in a comment by

Franco Modigliani reveal a reasonably stable relation between the rate of addition to capacity and the rate of utilization of capacity.

The three papers of Part III tackle the problem of explaining capital formation. In the first paper, Ruth P. Mack is concerned with business behavior resulting in expansion and contraction of inventories and its impact on cycles. While changes in aggregate inventories as a whole reinforce cyclical fluctuation, investment in inventories is characterized by great diversity within the aggregate. It is the author's position that knowledge regarding the behavior of particular inventories is necessary to gauge even very approximately the impact of inventory investment on the economy as a whole. In the next paper, Franco Modigliani investigates business reasons for holding inventories. In the final paper, Robert Eisner presents a review of the contributions of interview techniques and other survey methods to the study of private investment. He also presents some of the preliminary findings of his own study, the final results of which were published prior to the publication of this volume.[1] Eisner is meticulous in noting certain limitations of the interview approach.

Worthy of mention is the short but helpful introduction by Modigliani. Perhaps the best evaluation of these papers as a group is the following statement made by Evsey D. Domar, one of the conference participants: " . . . we should compare their results not with those obtained by some ideal method—here the disparity is of course great—but with what can be done by some reasonably practical alternatives. So viewed, these papers appear to be steps in the right direction, but in a long journey."

Capital Formation in Residential Real Estate: Trends and Prospects. A Study by the National Bureau of Economic Research. *Leo Grebler, David M. Blank, and Louis Winnick.* Princeton: Princeton University Press, 1956. Pp. xxx, 519. $10.00.

ROBINSON NEWCOMB, *Robinson Newcomb Associates*

THIS is, without qualification, the most intensive and authoritative report on the subject of housing since The Twentieth Century Fund's study *American Housing*, published in 1944.

The study is limited to an exploration of capital formation in privately built houses only. It does not, as did The Twentieth Century's Fund's study, explore the nature of the building or financing firms that are involved in the process of underwriting and erecting houses. It does not examine land problems closely. It discusses social factors, the impact of housing on communities and on the economy, and the impact of the economy or communities on housing, rather briefly. It is confined, as the title says, largely to capital formation associated with private house-building, from 1890 to 1950.

The authors try not only to delimit and then measure their problem, but also to find strategic factors which have influenced private residential capital formation. That is, they attempt to assess the implications of what they are describing and measuring. That is highly commendable. Some authors hesitate to do more than describe the facts they have assembled, leaving to the reader, who may know much less than the author, the task of distilling significance out of the facts.

Finally, the book makes specific predictions about the future. Scientific data have not reached the point of greatest value until they can be used for predictive purposes. Grebler and his coauthors, Blank and Winnick, believe their data are firm enough, and can be well enough interpreted, to enable them to make predictions. Let us hope

[1] Eisner, Robert, *Determinants of Capital Expenditures: An Interview Study*, Studies in Business Expectations and Planning No. 2, Bureau of Economic and Business Research, University of Illinois, Urbana, 1956.

that an increasing proportion of the reports of the National Bureau of Economic Research can, as does this book, contain one or more chapters outlining what, in the opinion of the authors, the facts reported mean for the future.

The first part of the book outlines the course of residential construction from 1889–1953, the growth of residential capital, population growth, and shifts in the geographical distribution of building. It then comes to a major conclusion that there has been a decline in real capital per dwelling unit, and that families may be placing more emphasis on other types of outlay, and less on housing. The authors feel—with justice, this reviewer believes—that Federal aids to financing helped revive construction in the 30's. They find, however, that the effects on residential capital formation since World War II—except for the early stimulus to rental housing—are difficult to appraise.

The second part of the book outlines in a similar fashion the growth of mortgage debt as the National Bureau staff have been able to unravel this historical mystery. It discusses the flow of capital funds into residential construction, and secular and geographical changes in the flow. It discusses also long-term changes in the cost and terms of financing, and the role of Federal aids in changing mortgage finance methods. The book finds that the Federal role can be much more clearly noted in its financial aspects than in its impact on housing as such.

The third part of the book looks to the future. The authors point out that with increasing incomes greater percentages of these incomes can go to new markets, so housing may get an increasing absolute flow of funds, but a decreasing portion of the total of all funds. They suggest, however, that, as incomes rise, the second house—such as a vacation or week-end house—may come within the means of an increasing proportion of families. Housing might, therefore, at some time in this development, take an increasing percentage of consumer income.

As valuable as any part of the study are the 17 well-documented appendixes. These cover such subjects as conversions and demolitions, starts from 1889–1953, expenditures in current and in constant dollars from 1889–1953, net capital formation, prices and costs, capital consumption allowances, capital-output ratios, mortgage debt 1890–1952; real estate bonds and debt, ratios of debt to purchase price, 1911–33; owners of mortgage debt, 1896–1952, comparisons of mortgage and long-term bond yields, etc. These bring together useful (and, in some cases, improved) data hitherto available only in widely scattered places, or not available at all. The book is worth the price to the student in the field for these appendixes alone, and for the discussions of the statistical weaknesses in some of the data.

Because the data are so carefully assembled and annotated, it is possible for the reader to form his own conclusions, to agree or disagree with the authors, point by point. This reviewer found himself disagreeing on some important points.

For instance, the series on net capital formation as developed in this study depends on estimates of gross capital formation, which must include outlays on new buildings, on improvements to the land, and on additions, alterations, maintenance and repair work done without financial outlay, and maintenance and repair work done with financial outlays. Then estimates must be made of depreciation, obsolescence, and losses, whether by design or as a result of catastrophe. Finally, adjustments must be made for changes in costs and prices. After going through each of these steps carefully, the study concludes, and tests this conclusion with discrete data, that the rate of net residential capital formation, in total, and capital used per dwelling unit, has been declining secularly for many decades. The study reports the rate of growth per decade as 47% from 1890–1909, 31% from 1910–29, about 0 from 1930–49, and 14% per decade from 1949–53.

There are, as the book carefully points out, many opportunities for error in selecting basic data, and in using the data. As an illustration of what this reviewer believes to have been basic errors, the book uses old official estimates of additions and alterations, rather than other available estimates which the government staff preparing the figures believed to be closer to the fruth. The government staff had been asked not to use the higher estimates, pending a revision of the entire GNP series, as to do so would increase the statistical discrepancy in the GNP series. Permission has finally been given to use the higher and more defensible series from 1945 on, and this portion of the data used is now officially as well as actually obsolete.

As the higher and more reliable series fluctuated in a fashion similar to the fluctuations of the older official series, the older series was satisfactory for the purposes of pointing out trends in the GNP. But for purposes of calculating net capital formation, the older series helped support a conclusion which may not be true and which may differ from some of the conclusions the new official series supports for the 40's.

Other illustrations of the possible errors in the figures used could be cited. For instance, during the war this reviewer was Director of the Division of Construction Research for the War Production Board. Our studies suggested that possibly 25% of building under way at the time in the nonmilitary sector of the economy was black market. Houses were built without authorization and were improved without authorization, and often without permits. The volume of capital formation apparently was much greater than that officially reported. Neither the official new construction or the official addition, alteration, or repair figures used during these years approached the true levels of capital formation.

This fact was illustrated by the sample census taken at the end of the war, and at intervals thereafter. The number and proportion of houses in good shape and with plumbing continued to increase, for instance, at a faster rate than official figures could explain. Over a million units standing in 1950, but built before 1940, were found by the Census Bureau to have had plumbing installed during these years, largely without benefit of reporting. The housing in 1950 was better than it was in 1940—though the figures used in this book show net capital formation—including new construction —as practically zero.

Such unofficial information as well as some official data, including the Census studies, suggest, this reviewer believes, that net capital formation was positive from 1940 to 1950, not zero. Other instances could be cited in which highly involved statistical calculations yield results which appear to be contrary to facts observable at first hand, or reported upon directly by others. But these illustrations of the weaknesses in the gross capital formation data in the last decade alone, suggest the difficulty this reviewer has with some of the statistics of the book.

The book is a pleasure to study because the facts used are clearly described. Each reader can check the facts used against those available to him—and know why he agrees or disagrees. He does not have to take the conclusions on faith.

This is an honest, competent, and valuable contribution. It probably will be at least another 13 years before it is replaced as the major source book on the subject, just as it took 13 years to replace The Twentieth Century Fund's study on the subject.

Dynamic Factors in Industrial Productivity. *Seymour Melman.* New York: John Wiley and Sons, Inc., 1956. Pp. xiii, 238. $4.75.

ISRAEL BORENSTEIN, *National Bureau of Economic Research*

IS IT possible to explain major variation in industrial labor productivity as a function of a few measurable factors? On the basis of detailed studies of industrial

operations and their cost and analyses of managerial decision-making, the author develops the view that there are two main variables that cause variation in productivity. These two "dynamic factors" in industrial productivity are: the decrease in the costs of machine operation relative to the costs of operation by manual labor, and the increase in administrative overhead. The author starts by putting forward "three related hypotheses." The first is that the ratio of labor to machine costs determines the degree of mechanization of a production process. The second is that the degree of mechanization "governs" output per production worker; hence output per production worker is, in effect, governed by the ratio of labor to machine costs. The third is that the growth of administrative overhead reduces the increase in output per production worker.

In Part I the author analyzes the process determining mechanization, and hence, productivity, in the British motor vehicle industry. He finds that the rise in the ratio of man-hour to machine-hour costs from 1938 to 1950 was the principal determinant of the intensified mechanization of materials handling that took place after the war. Other factors were either unimportant or closely related to the changes in alternative costs. Thus, an increase in output per labor unit or in the degree of mechanization was not an end in itself to management. The technical knowledge and capital required for mechanization were also available in 1938. The author stresses that some of the reasons given by management for mechanization, i.e., shortage of labor, shortage of floor space, and oversized inventories of work in process, are only subsidiary to alternative costs changes. Increased mechanization could only be an acceptable solution with the existing alternative costs ratios. The implication is that, had these ratios remained the same as they were in 1938, solutions other than mechanization of the production process would have been found. These characteristics obtained for American as well as British-owned firms.

Part II is devoted to proving the third hypothesis. Administrative personnel, which is represented in this book by the number of salaried employees, is shown to have grown faster than production personnel, represented here by the number of wage earners, in the British motor vehicle industry, as well as in British manufacturing as a whole. As a consequence, the increase in output per person employed was lower than the increase in output per production worker. Turning to the results of his industry survey, the author attempts to show that the increase in administrative personnel was traceable mainly to the extension of management's functions arising from changes in financial, commercial, and production problems. In solving its problems, management preferred ways that also extended the scope and intensity of administrative functions. These included: raw material supply, cost control, tax accounting, labor recruiting, industrial relations, export sales, etc. Government regulation, however, was not found to be a major cause for the rise in administrative overhead.

Part III demonstrates variations and changes in statistical indicators which could, if taken together with the conclusions inferred from the industry survey, be interpreted as supporting the three hypotheses. In the U. S. motor vehicle industry, as in the British, the greater increase in the cost of labor over the cost of machinery has been accompanied by increased mechanization. The ratio of labor to machine costs has always been higher in the U.S. motor vehicle industry than in the British. Correspondingly, there has always been a higher degree of mechanization of material handling in the U.S. automobile industry. The situation is similar in U.S. and British manufacturing industries as a whole. In both countries, in the last generation, there has been an increase in the ratio of labor to machine costs (measured directly, and also by the number of KWH purchasable at the cost of one man-hour) and an intensified mechanization (measured by the increase in horsepower capacity installed per pro-

duction worker). In view of management's sensitivity to changes in alternative costs (demonstrated in Part I), these two trends according to the author support hypothesis one.

The parallel increase in output and in installed horsepower per production worker during this century is taken to support the thesis that changes in man-hour output depend on changes in mechanization. As additional evidence, the author finds a positive correlation between data on installed horsepower per production worker and Colin Clark's data on man-hour output for various countries and different time periods.

Of greater interest than these correlations are the author's efforts to show statistically the connection between output per worker and the ratio of labor to machine costs (again measured by the number of KWH purchasable at the cost of one man-hour). He finds a positive correlation between these two factors in an industry by industry comparison of seventeen U.S. and British manufacturing industries. He finds an even stronger positive correlation between the two factors in a comparison of data in various countries and in different periods. An outstanding feature of this comparison is that, for the same ratios of labor to machine costs, labor productivity is much higher in the U.S. than in other countries.

The author examines the relation between productivity and administrative overhead by comparing changes over time and variations among countries. In the U.S. too, although not quite as sharply as in Britain, the proportion of administrative personnel has increased over time. Among industries there is no observable correspondence between changes in administrative personnel and changes in output per production worker. Similarly, comparing data for the U.S., Britain, and the U.S.S.R., the author finds that the shares of administrative personnel in the total do not vary considerably among the three countries. Differences in output per man-hour, however, are substantial.

In Part IV the author suggests how the increase in cost of labor relative to the price of machinery can be taken into account to improve the economic design of industrial plants, as well as to make more efficient decisions on machinery replacement. A chapter on implications for business emphasizes the impersonal, mechanistic nature of the process of making decisions and the significance of statistical data on wages and machinery costs.

The author's methods of finding the causes of productivity variation determine both the achievements and the limits of this study. The author demonstrates that a large part of the variability in productivity could be explained by the variation in the costs of labor relative to machinery. Taking the given cost relationship and the management problems of an industrial firm as a starting point, he is not concerned with the determinants of management cost or with the problem of how productivity gains are divided among the population (page 5). However, in general, all other conditions being the same, technological advance and increased output per labor unit increase the price of labor relative to the price of reproducible commodities (this is true at least in free-market economies). Just as it seems quite obvious that the ratio of labor to machine costs is among the factors determining the degree of mechanization, so does it seem apparent that increased mechanization will, in turn, increase the price of labor relative to machine costs. This, in turn, contributes to further mechanization and technological advance. Under usual market conditions, management's reactions to changes in alternative costs take the form of planned action. The impact, however, of this planned action on alterntive costs is mostly outside the realm of management. The author's view that more management is not necessarily better management is certainly a truth too often forgotten. Nonetheless, it seems to this

reviewer that the author carries too far his contention that increased administrative overhead bears no relation to the increase in output per production worker. Among the many factors necessitating an increase in administrative overhead, technological changes that yield higher output per person may well be important. Similarly, although output per production worker depends to a large extent on innovations requiring a higher administrative overhead, these innovations are not the only determinants. This multitude of interrelationships, together with the relatively small (but nevertheless indicative) variability of the ratio of salaried employees to wage earners, seems to be the reason why the author was unable to discover the relationship he was looking for. The author was perhaps too quick to dismiss as insignificant the ratio of salaried employees to wage earners, which was smaller in British manufacturing than in U.S. (20.0 compared to 22.2 per cent). He assigned, on the other hand, too much importance to the vague and relatively high figure (20 to 25 per cent) for the U.S.S.R. Institutional differences between the Soviet and Western economies are too large to make such a comparison possible. A relatively higher administrative overhead may be a permanent feature of the Soviet economic system.

In order to prove his point, the author relies all too readily on correlations of time series where trends are involved. In his international comparisons he uses data for the same country but for different time periods in which trends are bound to have some effect. This, of course, detracts from the significance of his results. Students of Soviet development will be struck by the inclusion of estimates of Soviet productivity and alternative costs along side data for free-enterprise countries. One view holds that Soviet wage levels and prices are normative and planned and are not representative of either the scarcity of labor relative to capital or its real cost. In this view, alternative cost ratios in the Soviet economy are only one of the Soviets' many planning devices to reach their economic goals. Among these goals, however, neither the minimization of the total cost nor the maximization of output per worker has been considered of great importance until recently. The achievement of planned production goals, which is put forward as the primary task, has frequently led Soviet management to neglect all other considerations. That sensitivity to "cost ratios" has remained substantial under such conditions would have to be demonstrated before changes in alternative cost ratios are accepted as determining changes in output per worker. The author relies for such a demonstration on recent studies of Soviet industrial management (B. Granick, "Management of the Industrial Firm in the U.S.S.R."; J. Berliner, "Soviet Industrial Enterprise," Harvard dissertation, 1954), which suggest that Soviet industrial managers respond to problems such as a relative labor shortage by wage increases, job up-grading, and devices similar to those used in the West. Such practices have certainly contributed to the wide differentiation of wage rates according to the scarcity of skills in the U.S.S.R. They may also have affected the relative income levels of the industrial and the farm population. It is doubtful, however, that the purely "nominal" (as opposed to "real") cost ratios of labor to capital, which were partly affected by these practices, had any important secondary effect on the centralized decisions or the relative use of labor and capital.

The author has described with great understanding the problems faced by an industrial firm and the ways in which the solution of these problems affects labor productivity. The author concludes that: "There is no unilateral line of action which any one interest group in the economy may take to raise productivity. Within the business process, productivity changes are the joint product of industrial employers and workers. Each group pursues its own occupational ways; each creates problems for the other. Productivity changes have been a derived effect of their interdependent problem-solving actions." With respect to future productivity in Great Britain, the writer

declares that: "In the foreseeable future the business process will continue to be the dominant form of production decision-making in British industry. Within this framework large increases in Britain's labor productivity await management's need to counter rising wages with productivity-increasing production methods." Readers will be interested in the policy implications of this book especially for countries carrying out national efforts to raise industrial productivity.

International Comparisons of Real Wages. Geneva: International Labour Office, 1956. Pp. vi, 89. $1.00. Paper.

MAURICE C. BENEWITZ, *City College of New York*

THE uses of real wage comparisons cover a wide range. They may provide rough measures of levels of living or real income. Both over periods of time and from place to place, real wage ratios may be used to compare remuneration of workers doing similar labor; and, using different wage and price series, real wage ratios may also be used to compare wages as a major element in worker income. Social scientists have used real wages in these and many other ways. A comparison which has had particular significance in recent years has been that between real wages in developed and underdeveloped areas. The change in the real wage ratio could tell us much about the progress of economic growth in expanding areas if a meaningful ratio could be devised.

The problems of devising such a real wage ratio or any other are the subject of this monograph by the staff of the International Labour Office. In a concise and useful way, it examines the aims of real wage comparisons, the ideal theoretical indices, the practical problems of data gathering and adjustments involved, and some recent international comparisons which have been undertaken. Though the monograph does not pretend to present original work (it draws heavily on past ILO and other studies), it does present much of the necessary theoretical and practical material in a single, short source which should be useful to those who compute, interpret, and use real wage comparisons. Not the least virtue of this report is its lucidity of style which makes it suitable as textual material even for undergraduate students of statistics.

Since the time and place adjustments necessary in price index construction are widely known, the emphasis of this study is upon the problems of gathering, weighting, and interpreting the necessary wage data. Price problems are the point of departure, however. Since an unadjusted price ratio is not usually feasible between countries, the manipulations necessary to create an adjusted price ratio and the way these manipulations affect the interpretation of the results are considered. Consideration is also given to recent econometric experiments which attempt to use indifference analysis or measures of the income elasticity of demand because classes of goods in the two areas compared are very different. (An interesting aspect of these experiments, such as that of the Bureau of Labor Statistics,* is that the groups involved in the experiments have similar standards though stationed in different places. The problem that faces us in measuring development is to make a meaningful comparison in the absence of this similarity.) Where price index adjustments must be great, it is noted that what can be established is the range between a limit using the Laspeyres formula and the limit achieved by using the Paasche formula.

But the most useful discussions in the monograph concern wage data and ratios. The average earnings figure to use (almost always preferable to wage rates) depends upon whether the real wage ratio computed is to be used to compare wages in similar work or to study wages as a major element in income. Differences in the weighting of

* United States Department of Labor, Bureau of Labor Statistics, *A Method of Measuring Comparable Living Standards in Communities with Differing Characteristics* (Washington, October 1953).

the wage term in the real wage formula are also examined and related to the type of comparison undertaken. Methods of allowing for fringe benefits and deductions from income are discussed. The study concludes that both contingent benefits and the deductions associated with them cannot usually be considered although other types of adjustments are necessary. Actual data available and their strengths and weaknesses are noted. The places in which special family living and other surveys would be particularly helpful are specified.

Recent wage studies undertaken by the Northern Countries of Europe, the European Coal and Steel Community, the Organization for European Economic Cooperation, and the Bureau of Labor Statistics are summarized and evaluated. Each study demonstrates how some of the problems and adjustments discussed in this pamphlet have been handled. It is clear that a comparison of real wages that meets most of the conditions set in this monograph requires in most countries special family living surveys to get proper weighting data for the price series and special collections of data to get average hourly earnings with similarly defined wage components for comparable sets of workers. Even then both the Laspeyres and the Paasche formulas will have to be used with the price data since consumption differences are usually too great to be ignored. (Since the authors of the monograph did not design the studies discussed, they were limited to noting how closely these studies conformed to the design this report held to be desirable.)

But neither the investigations examined nor the theoretical discussions in this monograph bring us closer to answering the problem of how to make meaningful studies between areas with greatly different socio-economic structures. To the extent that economic development and its measurement is one of the basic political, economic, and social problems of our time, the lack of adequate tools for comparing the progress of workers in developing areas is a major deficiency. This is, of course, a deficiency in our scientific apparatus rather than in this study in method. Studies over time are possible in growing areas so that internal improvements in wages can be measured meaningfully. But the larger task still remains; our current interest in development stresses the need for comparing change in differing as well as similar cultures if we are truly to measure even the material and tangible aspects of progress.

It is an achievement to have improved our comparisons between somewhat similar communities, and it is useful to have summarized the methodology involved and the pitfalls to be avoided. To say that we could wish for more is not to deprecate the amount of useful information on methods, problems, and interpretation of international real wage comparisons contained in this ILO study.

The Structural Interdependence of the Economy. *Tibor Barna*, Editor. New York: John Wiley & Sons, Inc., 1956. Pp. viii, 429. $7.50.

PAUL G. CLARK, *Williams College*

THIS book is the report of an international conference on input-output analysis held in Italy in 1954. A substantial editorial job has been done on the original papers, however, so that the book is noticeably more complete and coherent than conference reports are wont to be. In fact, for the general statistician who wants to know what has been going on in the input-output field, the book is a very useful summary. Such a reader is likely to come away with three main impressions.

First, the input-output approach has attracted serious research interest in quite a number of European countries, though their experiences have been diverse. There are six very interesting chapters on these national experiences. For Denmark, two 30-industry input-output tables are presented for the first time, and applications to tracing the effects of price changes are illustrated. In France, statistical research

preliminary to constructing a table has emphasized financial flows as well as real inputs. In Italy, a number of concrete applications have been made—illustrated here by a study of the repercussions of declining textile exports—and research is continuing to include such refinements as income elasticities and marginal capital requirements. For the Netherlands, explicit input-output projections have become less useful as post-war shortages and hence governmental controls have disappeared, but research continues to determine appropriate parameters for aggregative projections and to relate aggregative results to individual industries. In the United Kingdom, research efforts have been focused on producing a truly reliable and detailed input-output table for 1948 (400 industries in the basic data), taking full advantage of the rich supply of statistical materials available for that country.

The Norwegian research program, reported by Per Sevaldson, deserves particular mention because it faces up to the main shortcoming of the original input-output approach—the use of proportional input coefficients. In principle, he states, the objective should be to establish a separate production function for each individual industry. The Norwegians are experimenting with a number of interesting devices—alternative sets of input coefficients depending on relative prices, separation of individual products in multi-product industries, distinguishing between variable inputs and overhead inputs.

Second, the general reader may be a little surprised to find that the input-output approach can be used quite flexibly in constructing a variety of analytical models. A notable example is the extension into linear programming models. Of the three chapters on this topic, the paper by Hollis Chenery is perhaps most revealing for the general reader. He presents a linear programming model for calculating the minimum capital cost of a country's economic development program, considering the possibility of importing rather than producing at home, but recognizing that imports must be paid for with exports from other industries.

Another possible extension is to include consumption demand as one of the "industries," depending on household incomes by way of appropriate consumption coefficients. Elizabeth Gilboy discusses this approach as it is being studied at Harvard, pointing out that proportional coefficients are clearly inappropriate, but suggesting that nonproportional linear coefficients, distinguishing different groups of households according to income level and perhaps other characteristics, may prove satisfactory.

Even within the traditional matrix of input coefficients there are many possibilities for flexible treatment. Duane Evans, in the course of his truly definitive discussion of input-output computations, points out a variety of ways of handling such tough problems as alternative output measurements (e.g., value or tonnage) and secondary products.

Third, the general reader will observe that there is keen interest in integrating input-output and national-income accounts to improve the reliability and usefulness of both. There are five chapters on this topic. In particular, Richard Stone outlines a general system of social accounts, from which either an input-output table or a national-income table can be derived by appropriate aggregations, providing two models similar in form but of course different in the problems to which they are applied.

The American Workers' Fact Book. *United States Department of Labor.* Washington, D. C.: Government Printing Office, 1956. Pp. 433. $1.50. Paper.

JOHN V. SPIELMANS, *Professor Emeritus of Economics, Marquette University*

THIS publication is the first of its kind issued by the United States Department of Labor. Whereas the bulletins, reports, and handbooks frequently prepared in

the various Bureaus of the Department concern themselves with the detailed study of particular phases of labor economics, the present publication is a compendium of all the many significant facts concerning labor in the United States.

In the words of the Preface, the book is meant "as a report . . . about American workers for the information of American workers." However, with its wealth of facts and figures the volume will doubtless be very useful also to employers and managers in their dealings with labor, and, as a compact reference book, to legislators and administrators, journalists and editors, and students of labor in general.

The tenor of the book, as is to be expected from a publication of the U.S. Department of Labor, is objective, refraining from taking sides in controversial issues, yet sympathetic to the aspirations of the workers, and basically optimistic as regards their ever brighter future. The style is agreeably vivid and direct.

There are 138 sections, containing 53 brief statistical tables and 43 charts, grouped roughly under nine chapters. Many cross references to the separately numbered sections conveniently take the place of an index.

The topics include labor markets, employment and unemployment, hours and wages and total earnings, standards of living and living-cost indices, productivity and total product, and labor's share in it; workers' education and training; labor unions, their growth and activities; collective bargaining pracitices and the scope and coverage of collective agreements; labor disputes, conciliation and arbitration and strikes. There are also two brief chapters on the participation of American labor organizations and of government in international labor bodies; and a brief historical presentation of the development of governmental agencies in the United States dealing with labor's problems.

Much space is devoted to labor law, both federal and state: on the one hand, the laws for the protection of workers (child labor, industrial safety and hygiene, workmen's compensation, fair labor standards, fair employment practices, and all the various social security laws), on the other, the laws regulating labor-management relations (Wagner and Taft-Hartley and their various analogues in the states; anti-injunction laws, and laws restricting the activities of labor unions, in particular the state "right-to-work" laws).

The discussion of these laws is of course limited to their essentials, but the book presents them in the context of their social background, purpose, and history, explaining their operation and social and legal significance in precise, nontechnical language. The device of the cross references among various sections proves particularly effective in this connection.

A few specific criticisms may be in order. One concerns some of the graphs. There are a number of instances of widely separated or awkwardly connected bar graphs (worst of all the positively weird chart 12) where, in this reviewer's judgment, far greater visual impact and intelligibility could have been achieved through single or multiple line graphs (of which many are, of course, used); or, in the case of some frequency distributions of quantitative data (e.g. of incomes, or of employment time) by frequency profiles. (See *JASA*, Vol. 43 (1948), pp. 96–108.) Also, a thousandfold error has gone unnoticed in that the unit in total-population graphs is given as hundred-thousands instead of hundred-millions (Charts 15 and 21).

Another criticism concerns one of the statistical averages used, the so-called "average weekly hours" (given in table 19 and chart 17). This average, which has long been used in the wage-hour studies of the Bureau of Labor Statistics, is peculiarly uninformative because it is obtained from payrolls by dividing the number of paid-for hours during the week by the number of workers on the payrolls during that week. Its value therefore depends on two factors of quite separate significance: on the one

hand, on the length of the "nominal," full-time working week; on the other, on the amount of short-time employment or of overtime. In the average these two distinctive factors lose their identity. Part-time employment is indistinguishable from a shortened nominal week, overtime from a lengthened nominal week. Fluctuating employment opportunities, which may be of interest as rough indicators of the state of industrial activity, thus merely blur and distort that most-significant trend in our industrial society, the reduction of the hours of toil required from the individual worker, that is, the trend of the nominal working week.

The distinction between nominal and actual weekly hours is, of course, well known, and occasionally one finds it explicitly made. Thus in an older study of the National Industrial Conference Board both averages are fully discussed, tabulated, and graphed: two line graphs in one diagram, the one labeled "average nominal hours per week" falling steadily from 55 in 1914 to 48 in 1930; the other labeled "average actual hours per week" fluctuating below the first, lying well below it in 1914, climbing up to nominal in 1922, and in 1930 falling ominously below it to 42 (*Wages in the United States, 1914-1930*, National Industrial Conference Board, 1931, p. 63).

Thus in conjunction with each other the two averages do give a rather clear picture of the changing working week. But if only one is to be given, the nominal work week is significant in its own right, while the actual working week, taken by itself, is not.

It is interesting to note that in a recent study the BLS has indeed abandoned the old "average weekly hours" and instead gives only the nominal weekly hours called "weekly work schedules," with very revealing differences among industries and parts of the country (*Wages and Related Benefits, 1953-1954*, BLS Bulletin 1175, 1955). It is to be hoped that the Bureau will continue this new practice, the more so since with the coming of automation the trend of the nominal work week will become increasingly important. (The United Auto Workers have announced that they will seek a four-day week in the automobile industry.)

The space given here to the discussion of this statistical average is in proportion to what this reviewer has long felt to be the importance of drawing attention to it, not to its importance in the book under review. For as merely one among a host of other numerical data it of course in no way affects the great value and merit of the work.

Appraisals of Supervisors and Attitudes of Their Employees in an Electric Power Company. *Floyd Mann and James Dent.* Ann Arbor: Survey Research Center, University of Michigan Press, 1954. Pp. 39. $.75. Paper.

EDWIN R. HENRY, *Richardson, Bellows, Henry, & Company*

THE purposes of this study were to compare the attitudes and opinions of employees about their supervisors with evaluations of the same supervisors by their superiors and to explore the feelings of the supervisors about their own superiors. The study was conducted in the eight accounting departments of Detroit Edison Company. The eight department heads each evaluated those supervisors reporting to them, using the standard Detroit Edison Management Inventory. The evaluations were expressed in a 6 point scale from "Immediately Promotable" at the top to "Unsatisfactory" at the bottom. Employees filled out questionnaires about their supervisors and the supervisors filled out questionnaires about their superiors.

In general, employees working under supervisors who were themselves rated high by their superiors described their supervisors as follows:

He is good at handling people. He is a man with whom employees feel free to discuss important things about their jobs. He is also one with whom employees feel free to discuss personal problems. He goes to bat for them. He pulls for both the company and the men. He lets employees know where they stand. He lets his employees work

pretty much on their own instead of supervising them closely. He holds group meetings where problems can be discussed. He is a "leader of men," "likeable," "reasonable." He is not "bossy," "quick to criticize," a "driver," or "unnecessarily strict." He gives recognition for good work done. In particular, he gives employees recognition by training them for better jobs.

Employees working under supervisors who were themselves rated on the low end of the scale were much less likely to describe their supervisors in the above terms. They tended to be described as more the opposite.

Supervisors who were rated high described their superiors as men who (1) let them participate in decision-making; (2) let them know what they thought of their work, and (3) frequently asked them for their opinions. Supervisors who were rated low did not describe *their* superiors in such terms; they too were more of the opposite.

These findings are in essential agreement with findings from other studies by the Institute for Social Research and others.

On Human Communication: A Review, a Survey, and a Criticism. *Colin Cherry.* Published jointly in Cambridge, Mass.: The Technology Press of Massachusetts Institute of Technology, and New York: John Wiley & Sons, Inc., 1957. Pp. xiv, 333. $6.75.

WILBUR SCHRAMM, *Stanford University*

COMMUNICATION is one of those crossroads in science where many pass but few tarry. Therefore, just as it is of broad importance that in the last ten years a few men have tried to raise empirical work on communication to the level of theory, so it is of broad interest that Cherry should try to put some of these theories into perspective one with another.

Because this is likely to be an influential book, it is necessary to try to say precisely what it does and does not. It is not a book for the "general reader," although it so claims (p. 3). Indeed, the idea that the general reader can sail over Fourier wave analysis (pp. 128 ff) is one of the highest compliments that mythical individual has ever been paid. Furthermore, the book is not particularly good in appraising "the relationship between the diverse studies of communication," which is a claim the publishers make for it. Its strength is not in pointing out insightful relationships or intersections, but rather in the preliminary but necessary job of setting down on the pages of one volume, strained through one mind, several of the most important approaches from different disciplines to the problem of communication.

Thus the book represents Carnap, Ogden and Richards, Bar-Hillel, and Morris, among others, on signs and meaning; Sapir, Bloomfield, Zipf, Miller, et al., on language; Paget, Gabor, Stevens and others on speech and hearing; Hartley, Shannon, Wiener, Weaver, and Fano on the statistical theory of communication; and still others, all with their own outlooks on communication and their own ways of examining and measuring it. What the book does not contain is an interesting and somewhat puzzling revelation to us who have been accustomed to think of social psychology as one of the broad avenues to the understanding of human communication. This may be illustrated from the bibliography. In 367 bibliographical items, Cherry has no reference to Hovland, Festinger, Osgood, Newcomb, Merton, or Lasswell, and only one minor paper by Lazarsfeld. All these would be thought of in this country as contributors to communication theory. On the other hand, he has 7 titles by Gabor, 7 by Carnap, 6 by Shannon, 6 by Bar-Hillel, 5 each by G. A. Miller, S. S. Stevens, and Cherry.

As these citations might suggest, Cherry himself comes to the study of communication through the hard science door. He is a specialist in *tele*communications. He is therefore most at home with the study of signs when they can be studied without reference to their meanings, as Shannon and Wiener mostly do. He is more at home

with syntactics than semantics, and more at home with semantics than pragmatics. This leads him virtually to ignore the problem of the effects of communication on human relationships, values, and behavior.

The chapters in which he treats the analysis of signals and the statistical theory of information are easily the most sophisticated and successful in the book. This is an exposition which can be recommended with an easy conscience to any social science students of communication who are able to read calculus, or able to make the long jump over a few formulas. With great good sense, Cherry proceeds to set right some misunderstandings which have persisted too long in this area. He clarifies the relation of Shannon and Hartley as founding fathers, and concludes, correctly I think, that Shannon's greatest original contribution to the theory is his remarkable theorem for the capacity of a noisy channel, in which he calculates capacity to be equal to $WT \log (1+P/N)$, W being band width, T the time interval, P the average power, and N the average level of Gaussian noise during that interval. Although the Hartley-Shannon-Wiener statistical approach is the most pervasive approach in the book, and more is done with it than with any other material to relate a theory to others, still Hartley makes no effort to use it as a unifying theory. This, too, I consider a wise decision. On the contrary, Cherry walks through the pages of this book holding up a danger signal to those who would press the analogy of the Shannon argument too far. He points out that "information theory" was made primarily for telecommunications; that it is not designed for systems that learn; that it is not intended for treatments of meaning; that it is not for "information" in any sense except "instructions to select." He is rather unhappy about some of the loose analogies that have been drawn between statistical information theory and human information, and warns that the concept of entropy should be transferred from statistical mechanics to human communication only with great care if at all. "Without wishing to seem too assertive," he advises that "in true communication studies the concept of entropy need not be evoked at all."

It is perhaps a thankless kind of book to write, attempting as it does to represent so many diverse scholarships. The social psychologists will point out that they are omitted. The experimental psychologists, the logicians and philosophers, the communications engineers, linguists, and phoneticians, will all find their own fields insufficiently treated and some others (in their opinion) over-treated. Cherry advises his readers not to read the sections on their own disciplines. This advice will, of course, be honored in the breach. But yet the performance will not be thankless. In fact, it is likely to be an important book and to be recorded as a milestone in the development of a field. Undoubtedly its chief service will be to direct communication scholars of one discipline to the writing and thinking of other disciplines which deal with communication. As such, it will enrich the work of all and lead toward the general theory of human communication which must inevitably come, though still in the far distance.

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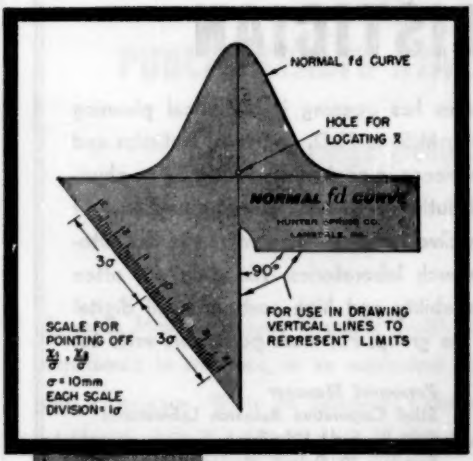

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